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Vol. VI

TRANSCRIPT OF RECORD

NARRATIVE STATEMENT OF PATENT EVIDENCE

Supreme Court of the United States

OCTOBER TERM, 1930

No. 378

STANDARD OIL COMPANY (INDIANA), STANDARD OIL COMPANY (NEW JERSEY), STANDARD DEVELOPMENT COMPANY, ET AL., APPELLANTS,

vs.

THE UNITED STATES OF AMERICA

APPEAL FROM THE DISTRICT COURT OF THE UNITED STATES FOR THE NORTHERN DISTRICT OF ILLINOIS

FILED AUGUST 23, 1930

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VOL. VI

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Burton's own oils are the round, black dots near the curve in the lower left-hand part of the diagram.

[fol. 967] You will see that they scatter and fluctuate, like any such series of actual determinations. But he does show a series of oils which have a certain typical gravity-viscosity relationship, and which, of course, tend to increase in viscosity as you see, as the Baume gravity drops off.

Or, in other words, as the oils get heavier in gravity the viscosity tends to increase.

Q. Now, you can take each of those points, can you, and verify it from the table?

A. Yes. For example, in a plot like this, any point on the chart represents either some actual or some theoretical oil, which oil has the viscosity found by passing horizontally to the left-hand scale, and, the gravity, found by passing vertically down to the lower scale.

For example, the first oil in Burton's table is 28.9 gravity, and 36 viscosity, and you will note that it comes above the point 28.9, on the gravity scale, and opposite the point 36, on the viscosity scale.

Similarly the position of each one of those points corresponds to one pair of values given in the table, and can be very readily verified from that table.

Q. Now, in addition to these nineteen round dots which form a rough curve, for the Burton oils, have you put on this chart the other points that Burton located on his other two tables, for his mid-continent oils and his California oils?

A. Yes, sir, I have. The small black triangles represent the California oils, mentioned in the Burton patent table. Those are six in number.

You will note that they, too, form a more or less characteristic series belonging in a certain family, and that we might draw a rough line, or curve, representing their general characteristics. And you will also note that this family of oils lies very much to the right and above the oils cited by Burton.

In other words, for a given gravity, which is represented by one vertical line, where the viscosity of a Burton oil might at 22½ gravity, be about 50 seconds, the viscosity

of this California oil is 138 seconds—a very great difference—bearing out the point which Burton mentioned.

Also, we get the same result if we compare oils of a similar viscosity, along a horizontal line.

The mid-continent oils cited by Burton, are also shown on this chart in the form of small black squares, five in number, and, again, you will note that those oils form a [fol. 968] fairly characteristic series, still further away from the Burton type of oils than were the California oils.

I also wish to point out that on account of the limitations of the Saybolt instrument, which does not give any readings below 30 seconds—that is, even the most fluid hydrocarbons will flow through in about that time—the curves necessarily flatten off to the right, somewhat above thirty seconds.

In other words, all these curves will necessarily break away to the right, near the lower part, because, for example, kerosene, from these fluids, of 45 Baume gravity will have a viscosity around 32 seconds.

All of these curves will characteristically tend to approach toward the right hand edge, but the unique character of the series of oils is shown by the point at which they rise rapidly in viscosity, which, for the mid-continent oil, is around 27 Baume, and for the California oils is around 22 Baume, and for the Burton oils is around 12 or 13 Baume—the viscosity of the latter oils not rising as rapidly as the other two series.

Q. And if you take just the prior art that Burton acknowledged in his patent, and then show his deviations away from that art, you would go down and to the left on the chart—is that true?

A. Yes. Burton's oils are characteristically below and to the left of the characteristic curves for the ordinary series of oils.

The Master: Is it your contention that a product having the exact relationship of viscosity and gravity, can be obtained in no other way except by the Burton process?

The Witness: We have not definitely examined many other possible ways.

I will say that it was never previously obtained, so far as we can find, and that so far as I know the only other

way to obtain similar products, even today, is to carry out the operations which, while not under the narrower limitation of the Burton product patent, include the essential features of holding a stock like gas oil, for a long period of time, at a cracking temperature, removing the heavier material and separating it from the heaviest tar, essentially as Burton did. These are really uniquely different hydrocarbons, a quite remarkable type of hydrocarbon—we don't know, even today, just what they are—but those, and other properties, show them to be quite different from ordinary petroleum hydrocarbons, or other hydrocarbon mixtures in the prior a-t.

As I shall show, the real oils that were cited in the prior [fol. 969] art, also fall among these similar oils which Burton cited, rather than approaching, at all closely, to the Burton type of oils.

The Master: What is in my mind is this: if some other manufacturer should find it desirable to make a product having a relationship of gravity and viscosity similar to what is described in this Burton patent, and should go about some other process to manufacture it, do you feel that the Burton product patent would enable you to prevent him from doing so?

The Witness: Well, there are, of course, other limitations which Burton mentions. For example, he mentions his hydrocarbons being of petroleum origin. He also mentions that they are substantially saturated. In other words, a person might make up such a mixture from benzol, which is of rather low viscosity, for its gravity, or might take some peculiar coal tar product, or brown coal tar distillate, and artificially make a series of oils which would be like Burton's, in this one respect, but which could be very readily distinguished from Burton in other respects. But I should say that unless you can distinguish it by one of these limitations which Burton has, himself, made, that it would come under the disclosure—of this patent, on a new product.

Q. Now, having gone through the plotting which Burton did, if you want to find out whether any other prior art oil had the gravity and viscosity characteristics of the Burton product, what would you have to do?

A. Simply, if it was not already in Saybolt units, or Baume units, convert it to those units and plot it here, and see whether it tended to fall among the characteristics of the Burton oil, or whether it fell, more or less, among the prior art oils, which Burton, himself, acknowledged.

Q. Now, tell us what is the significance of the Burton mathematical equation which is mentioned in his specifications and in his claims?

A. Instead of including a large scale plot, along with the patent, Burton chose another common expedient, which is quite familiar, of expressing the unique relationship between viscosity and gravity, by means of an equation.

That is, for convenience in handling the equation, we will call viscosity X, and gravity Y, and then an equation simply is a method of stating a certain relationship between X and Y.

It turns out to be rather a complicated form of equa- [fol. 970] tion, but it is an equation meant merely to represent this unique relationship that he is talking about—just as a plot might have done it.

We can take this equation which represents a certain relationship between gravity and viscosity, and draw a line which represents that equation.

In other words, this line, shown here, is the curve of the Burton equation—or, the Burton equation is the equation which describes exactly that curve, which I have drawn.

That is done simply by solving the equation for values of viscosity corresponding to any assumed value of gravity. If, for example, we want to find where the equation comes on the 25 Baume line—we put in the 25 Baume figure, in the equation, and solve it to find the corresponding value of viscosity, which we find is approximately 47.

We get similar points from the equation, at frequent intervals, and then draw through the points the curve which represents the equation.

It will be noted that here the equation is, possibly, not quite as close to the point representing Burton's actual oils, as one might desire. Of course it is impossible, with any series of experimentally observed points to get all of them to agree with the equation. All we can do is to get what we call a fairly representative line, to show the general trend.

In this case Burton's equation comes somewhat closer to the prior art than do the bulk of his points.

This practice of representing a curve by an equation is a very common practice in mathematics.

Q. And to a mathematician what is the significance of the equation as indicating the curve?

A. The equation is simply an easy and simple method to him, at least, of indicating the curve. Some of us rather prefer the curves for ease of visualizing.

Q. Now, please discuss the mathematical and graphical deviations of various observed points, from the line of Burton's equation?

A. As I say, this curve passes through all points which exactly satisfy the equation. Any other point on the chart will, therefore, if we substitute its values of gravity and viscosity in the equation, not satisfy the equation, but show a remainder left over, which we may call a deviation from the equation, as plaintiff's expert has done.

[fol. 971] The Master: If it steps out one degree in either direction, it is outside?

The Witness: It is outside of the exact equation. In fact, it would be almost impossible to get more than one in a thousand experimental points, which would be exactly on the equation. The equation is just the way we can represent the general trend of the system.

What we find is characteristic of this Burton equation is the deviation of his actual oils, which you will note, in all but one case, fall below the line, and as indicated, have deviations varying from minus 422 to minus 11604, for the point which, in a mathematical sense, at least, is furthest from the curve.

There is one point which is shown as coming just above the curve, which gives a small positive deviation—I say small, because the magnitude in any mathematical equation, is purely relative—a small deviation of + 1459, according to my calculations.

What we can say, then, with regard to the curve representing the equation, is that the Burton oils are characteristically as shown by his own specifications, oils which show either very small positive deviations, or negative deviations from this equation—up to ten or twelve thou-

sand. These would be fairly considered as within the purview of Burton's description.

Q. Because these are actual oils which he describes?

A. Yes, sir, as being new and unique in this viscosity relationship.

Q. And of which the equation was stated as being an approximate representation?

A. Yes, it is meant only to be approximate, of course.

Q. What is the magnitude of the deviation from the Burton equation, of the prior art oils which he, himself, cited?

A. The one which showed the smallest deviation from his equation is one of the California oils, with a viscosity of approximately 146, which shows a positive deviation of 132750—in other words, a positive deviation nearly a hundred times as great as any of Burton's oils.

Q. Please point that out to the Master. That oil which has the least deviation of any prior art oil that he names in his own patent?

A. (Pointing.) Yes, sir, and you will note that it comes, measuring in this way, among the closest to the curve. The [fol. 972] deviation, as measured mathematically, and as measured graphically, will not exactly correspond in the different parts of the chart.

The other oils cited by Burton all have greater deviations than these, the maximum being another one of the California oils shown at the top of this chart, which gives a positive deviation of 497961.

I may add that these deviations are approximately the same as plaintiff's expert finally gave, although they do vary by matters of a hundred or more, which is probably due to differences in the extent to which the figures are carried out—or the number of significant figures used. Mine were done mainly on the calculating machine, some of them being checked by ordinary calculation. But I am perfectly willing, as far as the variations in the figures are concerned, to accept Dr. McKee's. The variations between our figures are not significant, as compared with the tremendous differences between Burton's particular oils and the prior art oils.

Q. Now, have you marked on this chart the deviations, plus and minus, for each of those oils of Burton's table?

A. Yes, I have marked on the chart all the prior art oils showing plus deviations, while all but one of Burton's oils show minus deviations.

Q. In other words, his actual oils outdid his equation in their difference from the art?

A. Yes, his actual oils are even more different from the art than is his characteristic equation.

Q. Now, I want you to refer to the Aisinman Publication, "Exhibit No. 118," which plaintiff's expert cited against this product patent, and compare, briefly, the method of producing Aisinman's oils, with that of making Burton's oils, and state what one would naturally expect as to the nature of the products that Aisinman would produce, as compared with Burton.

A. Aisinman's oils are merely the heavier products of an ordinary coking distillation, most of them being lubricants, made in the ordinary coking operation, without pressure, and starting with crude oil. Whereas, Burton's, as I have already pointed out, are a type of hydrocarbons, not present in ordinary crude oils, so far as we can determine, and made by prolonged cracking under pressure, and at high temperature, of gas oils, form a new type of hydrocarbons, quite different from what we get from ordinary crude.

[fol. 973] One would, therefore, expect Aisinman's oils to differ from Burton's in the same way that the other prior art oils differ.

There is no reason, whatever, to think that Aisinman would get anything but a typical series of oils, made from mid-continent, or whatever type of crude he was working on.

Q. That Aisinman Publication—as I recall it—and I want you to correct me if I am wrong—is merely a description of the ordinary coking still operation, in its early days—is that right?

A. Yes.

Q. Now, do the actual oils disclosed by Aisinman, have the gravity-viscosity relationship which you say you would naturally expect?

A. Yes, they do—they fall quite closely in the series of oils which Burton gives, except that Aisinman gives one oil with very light gravity, which comes somewhat out of

the range that Burton was describing, but even that is of high viscosity for its gravity.

Q. Now, have you plotted on the chart the gravity and viscosity of the actual Aisinman oils? I don't want you to discuss them but just state whether they are there.

A. Yes, sir, I have.

Q. And they are shown, are they, by the hollow black triangles on the chart, with solid lines?

A. Yes. This oil (indicating), this oil, this oil, and this oil are the actual oils which Aisinman describes in his reference, and you will note, that they form part of the series which is characteristically different from the Burton series, and quite close to the prior art oils which Burton, himself, cited.

Q. Plaintiff's expert indicated, however, that some of the Aisinman oils came very close to Burton's equation—how was he able to reach such a conclusion?

A. Aisinman speaks, on page 1489 of the Book of Exhibits, in the last three lines, of oils varying in specific gravity, from .8 to .9, and in viscosity from 2 to 4 Engler.

Of course that means to anyone skilled in the art, that he had a series of oils varying from one oil with a viscosity of 2 and a specific gravity of .8 up to a viscosity of 4 and a specific gravity of .9, and those two oils are represented by the 2 hollow triangles which I have mentioned, on the chart. Undoubtedly he had other oils in the general range between these limits.

Q. That is this one would have a viscosity of 2?

[fol. 974] A. Yes, and a specific gravity of .8—the one on the right of the chart—and this one would have a viscosity of 4 and a specific gravity of .9—somewhat heavier—and he means to disclose, undoubtedly, a series of oils ranging in that general direction.

Plaintiff's expert's conclusion that Aisinman discloses oil which came somewhere close to Burton was based on an entire misapprehension of this reference, in which he indicated that Aisinman was disclosing not only the oils which I mention, which are quite clear to anyone skilled in the art, but that he also meant to indicate oils with the lighter specific gravity and the higher viscosity, and oil, also, with the heavier specific gravity and the lower viscosity.

In other words, he was crossing, or switching the ends of the series that Aisinman actually discloses.

The best way to make clear what this means, or amounts to, is to suppose that a man made a statement that he had a series of children varying in age from 2 to 10 years, and varying in height from 3 to 5 feet. Now, anyone would know, of course, that he meant to disclose that his two year old child was about three feet tall, and his ten year old child was about five feet, and that his other children varied in between, in height and age—along a more or less normal curve. Whereas, according to the method of interpretation used by Plaintiff's expert, that father would be held to disclose that he had a child two years old and five feet tall, and another one ten years old and only three feet tall—this resulting from switching the ends of the reference.

In other words, that method really creates an imaginary dwarf and an imaginary giant, neither of which were meant to be disclosed, at all, by the reference. The imaginary dwarf, of course, automatically has a low viscosity for its gravity, and hence, we find that the imaginary dwarf oils series, are those which seem to come rather close to Burton's oils.

Q. Have you plotted on this chart the imaginary oils that Dr. McKee conjured up here?

A. Yes, I have. In the Aisinman reference I have plotted out the two actual oils by two solid line triangles. The imaginary dwarf oil, having the viscosity of the light oil, and the gravity of the heavier oil, supposedly, is the dotted triangle at this point, which, being abnormally low—far lower than Aisinman's thought, or disclosure, does happen to come fairly close to the Burton equation.

[fol. 975] The Master: And would you say that it came within the disclosure of the patent, here?

The Witness: That would be a matter of argument, I would say. That shows a plus deviation of 37412, whereas the nearest Burton oil shows a plus deviation of only 1459. It comes much closer than any actual oil, and it is largely a question of argument as to just where Burton's precise line of demarcation, would come. It would be a fairly close reference.

Q. Now, can any such oil with those properties, be gotten by the actual process that Aisinman describes?

A. No, it cannot.

Q. Now, take the other imaginary oil that plaintiff's expert conjured up—the one in the upper right hand corner of the rectangle—and tell us, in words, what that would actually be?

A. That would mean that we had an oil with a gravity of a little over 45—which I would call a—which had a viscosity of 135 seconds—in other words, in the light lubricating oil range, and, of course no such combination has ever been seen, and I don't think it can be produced by any means.

Q. And in order to switch those ends he has to make two oils which are imaginary?

A. Yes.

Q. And neither one of them is one that an oil man ever saw?

A. No.

Q. Now, did plaintiff's expert adopt this same expedient of switching ends of series, or any other reference?

A. Yes. Every time there is a series of oils cited, plaintiff's expert adopted the expedient of switching the ends, and every time he thus created an imaginary dwarf oil, which came closer to Burton than any of the actual oils cited.

Q. And, also, what you have called an imaginary giant oil, on the other side and further away?

A. Yes. The other two cases are the references in Aisinman, mentioned on page 1490, and the reference in Riebeck.

Q. Have you plotted the other two imaginary oils that he took from Aisinman?

A. Yes, the other two imaginary oils that he took from Aisinman, are again arranged in the rectangle, with the two real oils—I point to the two solid triangles, which rep- [fol. 976] resent the higher viscosity actual oils of Aisinman, and to the imaginary dwarf and the imaginary giant oils.

Here, the fact that the gravities of the two real oils are rather close together, means that the switching of the ends does not produce as patently an absurd a result as before. But when we consider that Aisinman might as well have

covered the whole range, from the lightest to the heaviest of his oils, we might have had a rectangle which would have given us one oil up in the upper right hand corner of the chart, and another oil down right among the Burton oils.

But, we might say, fortunately, Aisinman covered a rather narrow range in his separate disclosures, rather than covering a wide range, which would make switching ends a much more patently absurd proposition.

Q. If you made the normal statement that one would make about the products from crude oil, and apply the same procedure that the plaintiff's expert did, you would get a gasoline as thick as cylinder oil, wouldn't you?

A. Yes.

Q. And, conversely, you would get a cylinder oil as thin as gasoline?

A. Yes.

Q. Now, will you just show us what he did with the Riebeck product oils when he switched the ends and got the two imaginary oils?

A. The Riebeck product oils are shown as hollow squares these in solid lines being the two real oils, and the two dotted hollow squares being the two imaginary oils.

Again, the imaginary dwarf of the series, comes the closest to the Burton reference, but since we are not covering a wide range deviation from Burton is still quite large. Whereas, the imaginary giant oil has a deviation of plus four hundred thousand.

Q. Near the close of his cross-examination, on page 800 of the Record, plaintiff's expert gave a corrected table, covering most of the references which he cited against this patent. Now, I wish you would point out, in that table, which of those oils are the real oils, and which are the imaginary ones?

A. The first twelve of these oils represent three series, two from Aisinman and one from Riebeck. For convenience I have lettered the oils in series from A to M—the letters A, B, C and D, therefore, represent one series, and E, F, G and H a second series, and I, J, K, and L, a third series. [fol. 977] In each of these cases the first and last oils of the group of four, are the real oils. The second oils in each series B, F and J, are the imaginary giant oils, made

by combining in high viscosity with low gravity, and the third ones, C, G and K, are the imaginary dwarf oils, which are the ones in each series which come closest to the Burton equation, being made by combining the low viscosity with the heavy gravity, to give a disclosure which was not within the scope of the true disclosure.

I have marked the imaginary giant oils, plus, and the imaginary dwarf oils minus, in each series.

Q. Plaintiff's expert told us, on cross-examination,—and you have confirmed his judgment—that all normal series of oils the gravity and viscosity go up together?

A. Yes.

Q. That is, oil gets heavier and thicker, as you take it off?

A. Yes.

Q. And in these groups the ones that you say are real, are the ones that are light and thin—I mean, the lighter and thinner—and the heavier and thicker.

A. Respectively, yes.

Q. That is, in the first one, 80 means that it is lighter than the 90, in weight, does it not?

A. Yes, sir.

Q. In the first group, A, B, C, and D?

A. Yes, sir.

Q. And the Engler viscosity of 2, means that it is the thinner oil?

A. Yes, sir.

Q. And the 4 means that it is thicker?

A. Yes, sir.

Q. So that you put together only the light weight and the thin viscosity, and the heavy weight and the thick viscosity?

A. Yes, using them in the same order that Engler does.

Q. Just as if he had said, "respectively" in his disclosure?

A. Yes, sir, that being absolutely implied to anyone who is familiar with oils.

Q. You would say "varying the gravity from .8 to .9, and the viscosity from 2 to 4 respectively?"

A. Yes.

Q. And that is all you have done here?

A. Yes.

[fol. 978] Q. And when plaintiff's expert departs from the "respective," you say the oils are imaginary?

A. Yes.

Q. And these same things that I have pointed out with regard to the first series, is exactly what you have done with the other two?

A. Yes, sir.

Q. Your selection of what are real and what are imaginary, depends on precisely the same considerations that I have just gone over with you, on the first four?

A. Yes.

Q. Now, I wish you would please refer to the discrepancies calculated for these twelve oils, by the plaintiff's expert, on the basis of the Bureau of Standards Conversion Tables, which have been admitted by plaintiff's expert as being the most accurate conversion tables available, and discuss their significance.

A. Confining ourselves, first, to the real oils disclosed, the deviations vary from oil A, which is plus 126000—and I will omit the last three figures, as not being of particular moment—to oil H which is the highest one, with 407,000 positive deviation.

In other words, these real oils of Aisinman and of Riebeck, cover practically the same range as Burton's own disclosure of the oils, which had deviations varying from plus 132,000 to plus 497,000.

Referring to the imaginary dwarfs, there are, in each case, closer than the real oils in the series, the closest having a deviation of plus 37000, which I have noted on the chart is the one that comes within a reasonable range of the Burton equation, not a real oil—but is not of particular significance—and no other of these twelve oils has a deviation lower than 126000.

The Master: What do you mean by its not being a real oil—do you mean it is merely imaginary—a merely imaginary compound?

The Witness: It is merely an oil whose properties are imaginary, made by distorting, or switching the description given in the reference. It is in exactly the same place as I illustrated by the example of a family—a father says he has children ranging from 2 to ten years age, and from three feet to five feet in height. He doesn't mean to dis-

close that the two year old child is five feet in height; and [fol. 979] the ten year old child is three feet in height. I call these the imaginary members of the family—just as these oils are the imaginary members of this family, not disclosed, or not intended to be disclosed by the man who was making the description.

Q. And they could not have been produced, in the case of Aisinman's oils, at least—they could not have possibly been produced by the the process described?

A. Certainly not, in the first series of oils, which contain the only one which comes at all close to the Burton equation.

Q. And the process described would produce, would they not, the oils which you have said were the real oils?

A. Yes.

Q. As one skilled in the art, would normally read this disclosure of Aisinman?

A. Yes.

Q. You have already discussed the character of the oils mentioned by Aisinman, stating that they are ordinary oils made by the ordinary distillation of American crude. What is the character of the two real Riebeck oils I and L that you have named in the table?

A. These two oils are the heavy tar left in the still, or withdrawn from the still, after Riebeck has cracked his heavy dark paraffin oils (which are, as I have shown, brown coal tar oils), taking off 25 to 30 per cent of distillate. In other words, they are residues left in the still after a light cracking of brown coal tar oils. They, of course, differ from the Burton oils in very many respects, starting with the entirely different types of compounds present in brown coal tar, as compared with those in Burton's saturated gas oil; also in the fact that cracking has been far less severe, taking off a comparatively small amount of a lightly cracked distillate, and particularly differing in the fact that they have not been redistilled from the black tarry residue, which would of course make them entirely unsuitable for Burton's twine oil, for example. In other words, they differ in very many ways, not being even a petroleum product, thought we would still have to examine to see whether or not they have a unique gravity-viscosity relationship of Burton oils.

Q. How about their saturation?

A. I presume they are quite unsaturated, and they probably contain quite an appreciable amount of oxygen compounds, in view of the character of the brown coal tar oils [fol. 980] from which they start; but I cannot state definitely in this respect.

Q. What is the sulphuric acid absorption of Burton's twine oil of this patent?

A. As a rule less than 20 per cent soluble sulphuric acid. As he himself says, they are highly saturated oils—surprisingly so, considering the method by which they are produced.

Q. Does merely finding gravity-viscosity relationship answer this Burton disclosure, or would you have to find other things?

A. No, we would have to find an oil which was essentially a petroleum product and one which was largely saturated, because he clearly imposed both of these limitations in his description.

Q. So far as the mere viscosity-gravity relationship is concerned, could you obtain that by mixtures of other things?

A. Yes, I imagine a mixture of some heavy tar with a product like benzol, for example, or other artificial combinations, might be made up which would come fairly close to having this gravity-viscosity relationship, at least as far as producing an individual oil which would meet the description. Burton, however, describes not only certain individual oils, but he says that these are cuts made by splitting up his twine oil as a whole, and I do not know of any way in which you could make up a mixture which would not only be similar to Burton's starting oil, but which when distilled into fractions would give the same typical series of fractions as he gets.

Q. By the way, how would these oxygen-containing compounds affect the gravity?

A. They would tend to increase the gravity quite markedly. All oxygen-containing compounds have high gravity compared to similar hydrocarbons.

The Master: You say that an oil could be made up to correspond substantially to the product of the patent?

The Witness: Could be made up by using non-petroleum products particularly.

The Master: Non-petroleum products?

The Witness: Yes. Benzol and tar, for example.

The Master: You mean benzol and coal tar?

The Witness: Well, benzol and coal tar or benzol and petroleum tar, if desired.

[fol. 981] The Master: Benzol is a possible product from petroleum, isn't it?

The Witness: Yes, it is a possible product from petroleum.

The Master: Let us assume that this patent is a valid patent, on the product and some person conceives that an oil of that kind is salable, and he makes benzol and he makes tar.

The Witness: From petroleum?

The Master: Yes, and he puts them both together and puts them on the market, although he has not followed Burton's process at all at any step. You would say that he could not market his product because it came within this patent?

The Witness: Well, you still have the limitation of Burton that his is a largely saturated product, which would not be met by that mixture, and you also have the description of Burton to the effect that if you take his oil which has a certain gravity and viscosity as a whole and re-distill it, you get a characteristic series of oils shown by the black dots on the chart, and you could not by any mixture of benzol and tar, for example, come anywhere near duplicating such a curve.

Q. If you had benzol and tar you would get off benzol and leave tar, if you tried to distill it, would you not?

A. Yes. You would take off a very low viscosity material, with a gravity, as I recall it, around thirty, and a large quantity of it.

The Master: This is limited to the use you make of it. You might not re-distill it at all, and it might be a marketable product.

The Witness: No, I am talking of part of the description of the product, the means of identifying this new product which he gives being the fact that if you take a sample of the oil and distill it you get this characteristic series of oils, which are unique in their gravity-viscosity relationships, and I do not know of any other way to approximate that

except by the Burton or similar cracking processes, starting with stocks similar to gas oil.

Q. This method of re-distilling an oil to determine its properties is a common and regular method?

A. Yes, used in identifying and testing oils quite generally.

Q. Just about as frequently as measuring its gravity?

A. Yes, almost.

Q. Even with the aid of the oxygen-containing compounds, how do these Riebeck oils compare in their gravity-viscosity relationships with Burton's?

A. Even with the aid of these oxygen-containing compounds the deviation of the real oils is 252,000 and 393,000, respectively, that of the imaginary dwarf being 174,000—all positive deviations, and very, very far from the Burton oils, which are almost entirely below the line of the equation, small negative deviations. I am referring now to the two real oils and the two imaginary oils in the residue of the Riebeck operation.

Q. I wish you would please summarize the effect of these references and their bearing on the Burton disclosure.

A. We find, considering all these references, that there is nothing which comes appreciably closer to Burton's oils than the oils which he himself cited and acknowledged as part of the prior art, save for the one imaginary dwarf oil created by switching the ends on the first Aisinman reference. Outside of that, the oils all fall in the same general area, with the same general deviations, as those oils that Burton himself cited as being characteristic of the prior art.

The Master: In what part of this patent is he citing prior art? I do not locate it myself.

The Witness: The two lower tables describe the prior art oils—that is, the regular oils from California crude and from mid-continent crude, which have certain viscosity-[fol. 982] gravity characteristics, and which you will recall I plotted as the solid squares——

The Master: They were such oils as came within his knowledge, that were made from California or Kansas-Oklahoma crude?

The Witness: By prior known processes, well known in the art.

The Master: He does not say anything about that, does he?

The Witness: I mean those are the ordinary products that are made by ordinary refining methods, "Some common petroleum oils made by other processes", as distinct from his new process.

Q. And those figures that he gives in those last two tables of the common oils, for the purpose of distinguishing, give the true and correct figures, as you know them, do they not, of the actual common lubricants taken from those oils?

A. Substantially so, yes.

Q. By ordinary refining methods. So that the art that the plaintiff's expert produced, in so far as it is real, differs just the same as that which Burton acknowledged in the patent?

A. Two or three references we have not yet discussed, but with regard to these twelve references I have already shown that to be true, and it is also true of the other references which I will discuss later.

Q. Have you plotted all of these deviation figures on your chart?

A. Yes, I have marked the numerical value of the deviations, which differ a little from those of plaintiff's expert, but I am perfectly willing to accept either figure, so far as that is concerned.

Q. And you have already explained how each of these four forms a rectangle, one diagonal of which is real and the other imaginary?

A. Yes.

This Riebeck oil M, the one which you put down, was that the last one on your table?

A. That is the last one in the table of plaintiff's expert, page 800 of the record.

Q. And that is the oil that is plotted down here by the square with the cross in the middle, is it not?

A. Yes.

Q. And that one is Riebeck's brown coal tar oil, the original raw material?

A. Yes.

Q. That is a real oil?

A. Yes. There is only one oil there. Instead of giving a range, he gives just one oil, so there is no chance for switching the ends, and we have just the one disclosure. That oil is, of course, not a petroleum product, and differs greatly from Burton's oils in not having been subjected to any similar cracking process, in being highly unsaturated, and containing a great many oxygen compounds, all of which differentiate it very, very clearly from Burton's product. By virtue of the fact that it has these oxygen compounds present, it does have a rather low viscosity for its gravity, but even then its deviation is about +55,000, which is some forty times as large as the only positive deviation shown by any of Burton's own oils. By referring to the chart it will be noted that even with the aid of these oxygen compounds, it is still well above the curve of the Burton equation, and still well above the oils which Burton disclosed.

Q. I want it perfectly clear that this is not the cracked product of Riebeck but the raw material that he starts with.

A. The raw material that he starts with for his pressure distillation.

Q. Brown coal tar oil itself. In the beginning of your testimony when you were discussing——

The Master: What difference does it make if it is a brown coal tar product if the first claim of your patent is sustained?

Mr. Wiles: Well, it is not a petroleum oil at all; it is a totally different thing.

Q. In the beginning of your testimony, Mr. Wilson, you discussed and contrasted the shale oils and the brown coal tar oils on the one hand, and the American petroleum products on the other, giving their broad general differences, pointing out how little of each was in the other. Do those facts that you then stated apply to this original Riebeck [fol. 983] starting material, the last one the plaintiff's expert gave in the table?

A. Those apply so far as my statements regarding brown coal tar oils are concerned. Of course, Burton was not here dealing with a crude petroleum, but with a cracked product. But he does definitely state that his oil is sub-

stantially saturated, which is true in point of fact, and in that respect it differentiates itself very clearly from products like the brown coal tar oil, which are highly unsaturated and contain a great deal of oxygen compounds, not being anything approaching a pure hydrocarbon mixture.

The Master: Those limitations, however, in regard to saturation, are not in any of the claims, are they?

The Witness: In the third claim I notice he says, "mainly of the paraffin series," and I have pointed out that the common usage of that term in the industry at that time, everything which was saturated was assumed to be a member of the paraffin series. Also he says in his specification, line 76, that his "mixture of oils is largely of the saturated or paraffin series".

The Master: Yes, but I am speaking of your claim now, not your specification.

The Witness: Well, I spoke about the fact that he says in the claim, "of the paraffin series", and I am showing by his specification that when he says "paraffin series" he means saturated, because he says, "largely of the saturated or paraffin series."

Q. That statement does in fact apply to the product of the Burton patent, does it not?

A. It is largely saturated, yes.

Q. And it does not ap-ly to brown coal tar oil?

A. No.

Q. And as a matter of fact the socalled brown coal tar is largely not a hydrocarbon oil at all?

A. Well, to a considerable extent, but I do not know just what percentage. They very frequently have ten to thirty per cent of what are known as tar acids, which are oxygen-containing compounds, and they also contain other oxygen compounds.

Q. And those would greatly increase its gravity, would they not?

A. Yes.

Q. On the Master's question, further, claim 2 of the [fol. 984] patent in a general way describes the process of manufacture. Will following that process result in a largely saturated product?

A. Yes.

Q. And the brown coal tar oil is not of that kind?

A. No.

Q. Whatever be its gravity-viscosity relationship?

A. That is correct.

Q. So that the gravity-viscosity relationship is only one of the criteria which Burton sets up, as you understand it?

A. Yes. He is describing a new product which differs in a number of characteristics from the prior art. He uses that as the most convenient method of distinguishing from ordinary petroleum oils.

Q. Plaintiff's expert in his direct testimony on page 370 and following cited several references against this patent and cited a number of oils which he claimed show very small deviations from the equation. Some of these were and some were not later mentioned in the cross-examination and the connection between his first reference and his later ones is not always clear, because of some unexplained changes that he made in his calculations. So I want you to take all and everything that he mentioned and consider it in detail, in either place. In the beginning I find on page 370 and page 371 that he cites an oil with a Baume gravity of 10.73 and a viscosity of 239, which he said gave a plus deviation of 12,144, and he says that is in the same range as the oils given in the patent, which deviate to the extent of 11,613. What are the facts on that reference?

A. The oil referred to here is really Riebeck's imaginary dwarf oil K, which later appears with somewhat different figures in the corrected table given by plaintiff's expert on page 800. The correspondence of the two oils might not at first sight be recognized, because the Baume given here as 10.73 really should be 17.45, this error being admitted by plaintiff's expert on cross-examination, page 728. As a result, the actual deviation shown in the corrected table of plaintiff's expert is plus 174,000, even though this is the imaginary drawf with the ends switched. I would also call attention to the fact that the statement that this deviation of plus 12,144 is in the same range as given by the patent itself, is not really accurate, because while it is true [fol. 985] that Burton does show deviations as high as 11,000, those deviations were all negative, the one positive deviation be-ng only a matter of 1400.

Q. So that in his original reference in his direct testimony plaintiff's expert not only switched the ends of the series, as you have explained, to make an imaginary oil, but

he also reduced the gravity seven points and thereby brought it somewhere near the equation.

A. The figures given there are in error in both of those respects, but were later corrected in the table, though the correspondence is rather hard to trace.

Q. Where do the real oils corresponding to this first citation fall?

A. They are oils I and L in the table, page 800, and have deviations varying from plus 250,000 to plus 296,000. They vary very far from the Burton description.

Q. The second reference to which I want to call to your attention, which he cites on page 371, is an article by Engler and Singer, which plaintiff's expert claimed to be an anticipation, because the oils they produced have quite low viscosity, and he said,

"By inference there is nothing abnormal in the way of the gravities of these oils." What have you to say on that reference?

The Master: Take an oil of 15 degrees Baume gravity and 130 viscosity, and another oil seventeen and a fraction Baume gravity and 139 of viscosity. Could anybody, any dealer in the products, without an analysis of it, discover any differences between the two products? Would it be seen in a tube or——

The Witness: You could not determine it visually, no; you could determine it in about five seconds with a hydrometer, however, an instrument which is used universally by buyers and sellers in the industry, to check up on the characteristics of the products. A great many petroleum products are bought with a specification of only one or two degrees Baume between the limits which must be met.

Q. I would like to go into this a little more in detail. When you buy oil and sell oil, Mr. Wilson, you measure and specify gravities, do you not?

[fol. 986] A. Yes, especially on the lighter products and on fuel oils.

Q. Everybody tests those as a matter of course with the same certainty that you would weigh any product on a scale?

A. Yes.

Q. By the ordinary hydrometer put into a graduate?

A. Into any suitable vessel.

Q. You also buy and sell products continuously on boiling point specifications, do you not?

A. Yes, products such as gasoline and similar light naphthas.

Q. And there is a regular method of measuring those things, by what we call a Bureau of Mines fractionation, is there not?

A. Yes.

Q. And making those inspections is routine in every shop that handles oil at all, is it not?

A. Practically so. Of course, there are some of the smaller dealers who do not pay much attention to specifications, but practically everyone that is equipped to make distillations, viscosity tests and gravities do buy on specifications, and they do determine gravities if nothing else.

The Master: Would a twine manufacturer in the use of the oil discover any substantial differences between those two oils?

The Witness: He certainly could, and I think in general he would. He would generally check up on that. He wants a heavy gravity oil and he is paying for that. It affects his pocketbook pretty closely, because the heavier the gravity the more pounds of twine he has to sell for a given amount of oil on the twine.

Q. The Master asked a question about analysis here and whether you would have to analyze. This regular Bureau of Mines fractionation that is made constantly, how long does it take?

A. If you have the apparatus set up, it takes about fifteen minutes to make a distillation. The gravity test is the simplest, requiring only a few seconds. The viscosity test requires a few minutes, and a complete boiling point curve, about fifteen minutes. Those are the three main tests that are used in buying and selling oils. There is very seldom any analysis as such except on some highly refined lubricating oils or medicinal oils, or certain products of that kind.

The Master: Take another thing. Following this process, when you are following a process of distilling of this nature, you have some fluctuations of temperature, do you not?

The Witness: Certainly.

The Master: And you have some fluctuations of pressure?

The Witness: Yes.

The Master: Is the product invariably of the same relation of degrees of gravity and viscosity?

The Witness: No, it shows characteristics variations of the magnitude which is in fact shown in Burton's own charts. You notice that these oils are not on a smooth curve, some are a little above the average, and some are a little below the average. You will find quite appreciable variations. But I know by actual experience that even a process that is as different as the Holmes-Manley gives a pressure tar which is in fact closer to these points on the average than the equation is to the points. In other words, we will get some variations in this general range of deviations of from zero to minus ten or twelve thousand, quite characteristically, due to fluctuations in the stock or the process or other things. But none of the fluctuations ever come anywhere near approaching the prior art oils. They will simply fluctuate back and forth in a rather narrow range below the equation.

[fol. 987] The Master: Mr. Wilson, when did you say that you went to the Standard Oil Company of Indiana?

The Witness: In 1922; the fall of 1922.

The Master: Then you have no personal knowledge of the commercial situation or the exigencies which brought about the alleged invention or discovery of this Burton patent we have been talking about?

The Witness: Not from direct personal knowledge, but from a study of the literature and from a discussion with many men, both inside——

The Master: We are not speaking of the technical art with relation to it; I am only speaking of the external circumstances and business exigencies, particularly with relation to the making of binder twine, for the binder reaper and mower.

The Witness: No, on that point I have no particular knowledge.

The Master: And no knowledge of the binder twine art and its requirements in respect to some find of a finishing product to finish the twine?

The Witness: Why, it is my understanding they want a certain amount of oil on there and they like to have it a heavy oil, but not too viscous.

The Master: That is just general, of course; that is assuming; but you have no special knowledge of that art?

The Witness: I have no special knowledge of that art, no, sir.

The Master: What do you know, if anything, of the production of paraffin oils prior to 1912?

The Witness: I am familiar with the general practice as it was carried out in coking stills which were then very much the same as now.

The Master: Paraffin oils were required commercially and were manufactured prior to 1912?

The Witness: Oh, yes, by very much the same methods as are used today.

The Master: Well, the product of this patent is a paraffin oil, is it not, in this product patent?

The Witness: Not in the sense in which the industry ordinarily terms it. It consists mainly of saturated hydrocarbons which Burton at that time, in common with the rest of the industry, called saturated or paraffin hydrocarbons, but even they would not have called it, I do not believe, a paraffin oil, because paraffin oils are characteristically oils containing paraffin or from which paraffin wax as such has been separated. In other words, the term "paraffin oils" does not refer so much to the hydrocarbons that are present as to the fact that they are made from paraffin distillate. [fol. 988] In other words, from the same distillate as contained the paraffin wax. Of course, the European use of the term "paraffin oils" is quite different, as I pointed out.

The Master: He selected the material to operate on as being the tar which contains the paraffin constituents?

The Witness: The saturated constituents, yes.

The Master: It distinguishes it as paraffin constituents, does it not?

The Witness: As a matter of fact, we today believe that there are practically no paraffin hydrocarbons in Burton's twine oil. In other words, that its unique character is due to the presence of other saturated compounds, probably ring compounds, but as I pointed out, the common practice then was to refer to everything which was saturated as being paraffin.

Q. "Paraffin" originally meant, I believe you said, without affinity?

A. Yes.

Q. What kind of affinity was referred to?

A. Affinity with sulphuric acid?

Q. So that the oils that did not go into solution in concentrated sulphuric acid were normally called paraffine?

A. Yes.

Q. And many of those we now know were not strictly of the technical, chemical paraffin series?

A. Yes. As shown by work in the last five or ten years.

Q. Now, is the oil of this Burton patent you are discussing, of a character which, according to that older phrase meaning without affinity for concentrated sulphuric acid, largely paraffin?

A. Yes. In that sense. We do not even today know just what kind of hydrocarbons are in this Burton oil. They are a very peculiar type of hydrocarbons which do not seem to be found any place else. We presume they have some form of ring structure, but we do not even know that for certain.

The Master: You do not even know that Burton was trying to get this particular oil at the time when he first developed it?

The Witness: I do not know what he was trying——

The Master: That he was constantly trying for it or whether it was just a cut of a process that he was using for the purpose of getting as much of the naphthenes out as possible.

The Witness: It may have been that he discovered the product in just making a study from a standpoint of scientific interest of the character of his pressure tar.

The Master: There is no use to speculate. I suppose Burton is still alive.

Mr. Wiles: I shall not ask him that question, because I take it as a matter of law, it makes no difference whether it is an accidental discovery or a conscious one.

The Master: I do not think it does, if it was a discovery. [fol. 989] Yes, there is no question but what these are a very unique type of hydrocarbon is this oil. As I say, the gravity-viscosity relationships are simply an easy manner of indicating something very fundamental involved.

The Master: You may go on with your examination. I will still continue to grope.

Mr. Wiles: When you get an oil that is as heavy as water, or nearly as heavy as water, and almost as thin—that is, exaggerating, but actually which approaches at all in that direction, a very heavy, very thin oil, you are clear off the track of all ordinary oils. This is a unique beast, and we know of nothing like it at all.

The Master: Still reaffirming, are you?

The Witness: Yes, sir, that is correct. Of course, it is quite a bit lighter than water by the time it gets down to that viscosity, but it is decidedly out of the range of anything that we know.

Q. The second reference which Dr. McKee cited on page 371 is the Engler-Singer article which he claimed was an anticipation, because the oils they produced have quite low viscosities, and he said:

"And by inference there is nothing abnormal in the way of the gravities of these oils."

What have you to say on that?

A. As I have already pointed out Burton, of course, never dreamed of trying to patent low viscosity oils as such.

The mere fact that an oil is of low viscosity does not bring it within the range of what he is considering. It is a question of whether it has an abnormally low viscosity for its gravity.

Therefore, when plaintiff's expert says about the gravities "by inference there is nothing abnormal in the way of the gravities", why, that at once indicates that it presumably is a normal oil with normal relationship between gravity and viscosity and therefore radically different from that of Burton, whose oils have a very abnormal relationship between gravity and viscosity.

There are many low viscosity oils, of course. Every gas oil, every kerosene would be an anticipation on that theory, [fol. 990] because they have low voscosities and there is nothing abnormal about their gravities.

The Master: In a paraffin oil is the viscosity low with reference to its specific gravity?

The Witness: No, it is high. The paraffin oils, you notice, are in the upper part of the chart. Even for gravities as much as 25 Baume they have quite high viscosity, viscosities of several hundred seconds; whereas Burton's oil for such gravities has a viscosity of less than fifty seconds. The ordinary paraffin oils are exactly what Burton is distinguishing from when he gives an example of the ordinary paraffin oils as he produced by the older processes to contrast with his own.

Q. Now, do you agree with plaintiff's expert when he says that "In fact, there is presumably nothing abnormal about the Engler-Singer gravities?"

A. The material that he has described is made from fish oil, and I do not know just what is normal for fish oil or for the products of cracking fish oil, and therefore I cannot say whether they would really be normal or abnormal. In any case, they are not petroleum products and they differ very widely from anything which Burton described.

Q. Now, in the next reference, Aisinman, on page 372, plaintiff's expert cites an oil with a specific gravity of 0.9 and an Engler viscosity of 2, which he says has a deviation of minus 8341 according to Battle and plus 8284 according to the Bureau of Standards table. Now, what are the facts about that oil?

A. In the first place, that oil is again the artificial dwarf of the series. That is, that is the oil with the specific gravity of .9 and an Engler viscosity of 2; whereas, what Aisinman really cited was oils varying from 0.8 to 0.9 in gravity and from 2 to 4 in viscosity, the 2 viscosity obviously referring to the .8 gravity and the 4 viscosity to the .9 gravity. But by combining the 2 Baume with the .9 specific gravity plaintiff's expert made the imaginary dwarf oil which comes closer to Burton than the real oils of the series. He here reports a deviation in the one case of minus 8341 and in the other case, according to the Bureau of Standards' table, of plus 8284. The first would, of course, put it right down among the Burton oils. That is [fol. 991] about the order of deviations that they have.

However, when you go to the corrected table this same oil, which is really oil C of the table, plaintiffs' expert recalculates to have deviations which this time are both posi-

tive; that for the Bureau of Standards being plus 37,254 and that according to Battle plus 10,899; which of course are quite different from those previously quoted. However, since this is only an imaginary oil it does not matter particularly how great its deviations are.

Q. In this case plaintiff's expert not only switched the ends of the series so as to combine the heavy gravity with the low viscosity which nobody in the industry would ever dream of, but he also made a mistake in calculation or at least he misstated the results of his calculation so as to greatly exaggerate the error that he made.

A. Apparently so.

Q. Now, what are the two oils which he cites on page 372 from Aisinman?

A. The next two oils cited are members of the next series. In this case he gives the deviations for two, the first which is the artificial dwarf which I called G, and the other of which is *is* the actual oil E. In this case he gives on page 372 a deviation of plus 42,874, whereas in the corrected table he mentions this same oil as having a deviation of plus 201,294; while the other real oil E which he gives on page 372 as having a deviation of plus 26,569, in his final table he gives as plus 285,222. This is not only an enormous change in figures but reverses the order. According to this, the real oil was closer than the imaginary oil, but in the corrected table the real oil is considerably further away from the imaginary oil, although all of them are quite far away.

Q. In this matter, too, the plaintiff's expert not only switched the ends of the series for one of the oils, but as to both of them he misstated the arithmetical facts.

A. Yes, there are some errors involved some place.

I might say that these calculations are not simple and it is much easier to see the whole situation by plotting the oils, in which case there is no chance to make a mistake, but [fol. 992] I believe plaintiff's expert testified that he had not plotted these results.

Q. When you say they are not simple, Mr. Wilson, it is merely a matter of carefully carrying out and the solving of an ordinary equation in simple algebra, is it not?

A. Yes, it is a matter of very careful work, not making mistakes.

Q. You have just got to multiply and add and divide simply?

A. With rather large numbers.

Q. And be careful you do not make a mistake?

A. Yes.

Q. Now, plaintiff's expert on page 372 was asked a question which reads substantially like the third claim of the Burton patent, as to whether any of the disclosures show a petroleum oil having a Baume gravity of 15 degrees and a viscosity of approximately 130 seconds measured by the Saybolt viscosimeter at 100 degrees Fahrenheit, and comprising a mixture of fractions mainly of the paraffin series and having a Baume gravity ranging from approximately 20 degrees to 10 degrees and the corresponding viscosity ranging approximately from 50 to 200", and he cited an oil of Aisinman as meeting this description. What is the fact?

A. This wording clearly refers to a composite oil which has a Baume gravity of about 15 and a viscosity of about 130 seconds, and as further describing the oil states that if you take it and distill it you will get a series of oils from light, low viscosity oils—(that is, comparatively light, low viscosity oils) up to comparatively heavy and higher viscosity oils, the ranges being from 20 to 10 in gravity and from 50 to 200 in viscosity, this corresponding to the left-hand two-thirds of the curve in the drawing.

Of course, there is no question here but what the 20 Baume, the lighter oil, corresponds to the 50 viscosity; the lower viscosity; and the 10 Baume to the 200 viscosity. That is made even more clear because he has reversed the magnitude of the numbers in order to make them correspond in a normal way. That is, he says from 20 to 10 Baume and from 50 to 200 viscosity. So that the first two go together and the second two go together, as you would expect.

Q. He says a corresponding viscosity also?

A. Yes, he also says a corresponding viscosity, so there is no question as to what that could mean.

[fol. 993] Now, the Aisinman reference is nowhere near —not within a very, very great distance of the specific oil or any of the oils which Burton cites. Plaintiff's expert said that the Aisinman oil had a Baume gravity of about 20 degrees, whereas actually, it is 20.65 gravity, and its vis-

cosity by the Saybolt scale instead of about 300 is actually 341.6 seconds. In other words, it is over five points lighter in gravity than Burton's specific oil, and yet instead of being less viscous it is about nearly three times as viscous as the Burton oil.

Q. I wish you would point that out, Mr. Wilson?

A. (Referring to chart.) This is actually the Aisinman oil H in the table of plaintiff's expert. That is, this oil to which I am pointing, with a little over 345 seconds viscosity, as compared with Burton's oil down in this region. It has lighter gravity and yet very much higher viscosity. No one could certainly say these bore the slightest resemblance to one another, and the only way in which I think plaintiff's expert could possibly have conceived a similarity is this time by switching the ends of the Burton disclosure.

In other words, just as the imaginary dwarf of the prior art oils comes nearer to the Burton curve than any of the true ones, so the imaginary giant of the Burton disclosure comes fairly close to the prior art. That is, if you switch the ends of this patent and put together the twenty gravity, which is the light gravity, and the 200, which is the heavy viscosity, you get something which is nearer to Aisinman, although even then it is still a long ways below the 341 seconds Aisinman oil, but that is not Burton's disclosure. Burton never did or meant to disclose or indicated that he had an oil of 20 gravity and 200 viscosity, which was only created by this same process of switching ends and taking the imaginary giant.

Q. And Aisinman, I think it has been clearly brought out is an ordinary oil made by ordinary processes, is it not?

A. Yes, by cooking crude petroleum.

[fol. 994] Q. Just the same kind of oil as Burton was referring to in the second and third tables given in his patent?

A. Yes.

Q. And it actually does in fact correspond to the oils given in those two tables very closely, as shown by the chart, does it not?

A. Yes, it is right in that general group.

Q. The last statement which plaintiff's expert makes in this connection is on page 373 with regard to whether any

prior art processes made petroleum oils by a method similar to that of Burton. This answer in the second paragraph of page 373 is not at all clear, and I judge that he first suggested Aisinman and then withdrew him, because of the fact that he does not distill under pressure. He then appears to say that Riebeck's residue from his process would produce essentially similar oils except that they had not been distilled at the last stage. What are the facts on this?

A. Of course, Riebeck's oils, as I have already pointed out, differed from those produced by Burton in starting with quite different types of oils—that is, the very dirty brown coal tar oils or oil gas tar or distillates from oil gas tar; Riebeck's oil also differed in the amount of cracking it had undergone, because he was mainly interested in making kerosene by light cracking; and the oils differed, further, as plaintiff's expert acknowledged, in the fact that Riebeck did not redistill his product to separate out the tar and get an overhead product, this omission of course, being fatal either for its use as twine oil or for its use as cycle stock.

Q. You used the term "overhead product", Mr. Wilson. That is a technical term in the trade. I wish you would state what it means. We may have to use it several times.

A. The term "overhead product" which I think I have already defined, is the common distinction between the part distilled off from an oil in a still and the part which remains behind. In other words, the operation of distillation generally results in a distillate or overhead product and a residuum which is left in the still.

Q. Is there any other evidence given by Riebeck himself which indicates any marked difference between his process and that of Burton?

A. Yes, one very striking example is given by these very gravity-viscosity relationships that we have been speaking about. It is of course evident that Burton starts with oils [fol. 995] which have a normal relationship between the viscosity and gravity—that is, they have relatively high viscosities for their gravities. By his cracking process he produces a new series of oils which are much lower in their viscosity for a given gravity, or much higher in gravity for a given viscosity. That is the striking and pronounced

change which he brings about, whereas Riebeck, on the other hand, starts with an oil which is nearer to Burton than his finished products, and goes in exactly the opposite direction, changing from a brown coal tar oil, which has a plus deviation of 55,000, to pressure tars whose plus variation varies from 240,000 to 297,000. This very marked change, going in exactly the opposite direction, is one of the best evidences in the world of the entirely different nature of the starting material and of the finished material and of the reactions which take place. I have already pointed out that——

The Master: See if I understand that. Suppose Riebeck, instead of choosing brown coal tar for his starting point, had been in a locality where petroleum tars and residues were more accessible than coal tar, and had pursued his same operation. You- idea is he would have gotten farther away from Burton?

The Witness: I do not know what he would have done in that case. Of course, the amount of his cracking was so different, and even then his charging stock would be different. So really I do not know which way he would go if he had taken crude petroleum and cracked to make kerosene.

The Master: I do not mean crude petroleum; I mean some of the residual petroleum.

The Witness: Well, the same thing is true. I do not know which way those would have gone. I am simply pointing out that as far as we have his disclosure, it was radically different from anything that Burton did. What he might have accomplished by taking a reduced crude I cannot really tell. Of course, if he had taken Burton's gas oil and cracked to make gasoline, he would have gone in the same direction Burton did.

The Master: If one skilled in the art had only before him all that Riebeck could teach him, and being requested to make some kind of twine oil, and being accessible to a region where tar from petroleum was to be found, could he with that teaching on that material have produced the required product?

[fol. 996] The Witness: I don't think he could. I don't know; I have never seen it tried. But I do not think he could.

The Master: Of course, what the mythical man skilled in the art could do is a very wide range, differing with the opinion and interest of the one who answers the question.

Q. If he had wanted to get an oil which was thinner for its weight—that is, a heavy oil that was thin—what would he have learned from the Riebeck disclosure?

A. He would have learned that it was better to take the brown coal tar oils that you started with, rather than the residues after the cracking operation, because the brown coal tars that he started with were actually quite thin, for their weight compared to most ordinary oils. The only indication that one skilled in the art would get from looking at Riebeck would be that according to that disclosure the cracking process gives a residue which, instead of being less viscous for its gravity, is actually much more viscous for its gravity.

Q. And is it not a fact that that change was made a particular merit of the Riebeck process?

A. Yes, that is true. Riebeck emphasizes that these paraffin oils are not suitable for use as lubricants and by carrying out this operation he produces good lubricants. He emphasizes that strongly. And, as this shows, he is describing here the highly viscous materials which he considers make good lubricants. Of course, Burton's oils are not in any sense satisfactory lubricants.

Q. And after Riebeck has taken this brown coal tar oil, which is a thin oil and not in the lubricant range, and has treated it by his cracking process, it has actually moved into the lubricant range, has it not?

A. Yes, it has.

Q. Just the opposite of Burton.

The Witness: Yes.

Q. I believe you quoted Dr. McKee's book on shale oil, where he talked about the contrast between cracking well petroleum and cracking oil of the shale type. I believe you quoted a part where he said that they were exactly opposite in their natures. How does this change of Rie-
[fol. 997] beck's raw material into a more viscous lubricating oil bear on that question?

A. I believe Dr. McKee's statement was that the results were opposite. That was more in respect to their saturation, however, whereas this is in respect to their viscosity. I do not know whether they are opposite in the same sense or not, but they are both in marked contract to the Burton operation.

Q. During his cross-examination Dr. McKee testified in answer to a question by the Master that one attempting to carry out the Dewar & Redwood or the Pielsticker process, or the Barbet process, might innocently produce the Burton twine oil product. What is your opinion on this matter?

A. I have already pointed out that all of these prior art processes, in the first place, started with quite different starting materials from those of Burton, and also they crack to quite different extents (that is, those which were operating by bulk pressure distillation and which would therefore have anything even remotely resembling Burton's tar). This difference in the starting material and difference in the extent of cracking cannot possibly fail to be reflected in the differences in the character of the pressure tar. But in addition to this, whenever the prior art speaks of the nature of this pressure tar it brings out these differences very clearly, and, as I have already pointed out, both Riebeck and Barbet speak of their pressure tars as containing desirable lubricating fractions; whereas Burton's pressure tar on account of this different starting material and different extent of cracking, contains as far as I know, absolutely nothing which would be suitable for use as a lubricant. As further evidence of the difference, wherever, like Riebeck, they do describe the character of their pressure tar, we find its gravity-viscosity relationships up among the prior art oils and not among the Burton oils—which is also further indication of the relatively light nature of the cracking. In addition to that, none of them disclose the essential step of re-distilling this pressure tar to get satisfactory light colored product, that being one important step in Burton's invention. Therefore, they never had anything which would have been suitable either as cycle stock or as twine oil.

The Master: The fact that they mention that their pressure tars contained desirable lubricants implied a redistilling to get those lubricants, did it not?

[fol. 998] The Witness: Apparently they got those lubricants by treating with acid, as near as we can tell from any references I have seen. They removed the undesirable materials by treating with acid, just as cylinder stocks are treated.

Plaintiff's expert also said that practically all of these processes would produce very similar products. If that is the case, then they would all be somewhat similar to those of Riebeck, who is the only man who did disclose the exact character of his tar, and that would mean that they would all be among the prior art oils rather than bearing any close relationship to Burton. Certainly there is not among them anything which would be at all close to Burton's process.

Q. I wish particularly in connection with the matters you have just mentioned, to call your attention again to the Barbet statement with reference to his pressure tar, in fact the heavy ends of his pressure tar. What bearing has that?

A. He states that it can be pressed for paraffin, whereas such an operation is absolutely hopeless on the Burton pressure tax.

Q. Plaintiff's expert stated on page 375 and he later reiterated under cross-examination on page 823 and following that the old shale oil lubricants are practically the same as Burton's twine oil. In support of this position he gave no actual data, but he stated that the Burton product owed its low viscosity with respect to gravity, to its high content of unsaturated carbons, which he believed, he said, was probably around 75 per cent for the twine oil, as it was in the case of the shale oil lubricants. What are the facts on that point?

A. Plaintiff's expert could hardly have chosen a more striking way to show the great difference between Burton's substantially saturated low viscosity pressure tar and the shale oil lubricants, in assuming, as he apparently did, that the low viscosity of Burton's material was due to the high content of unsaturated hydrocarbons. He not only showed a complete lack of knowledge of the product, but disregarded Burton's very clear statements as to the character of the product being substantially saturated. As I have already pointed out, treating with an excess of strong sulphuric acid only removes fifteen to twenty per cent of the oils from this, and the product that is left after that still has his unique

gravity-viscosity relationship, showing that it is not in any sense due to its content of unsaturateds but due to the peculiar character of the saturated hydrocarbons which are present.

In contrast, plaintiff's expert said in one place that shale lubricants are from 50 to 80 per cent unsaturated, and in another case, speaking of the refined shale lubricants, that they are about 75 per cent unsaturated. Even if the shale lubricants did prove to have similar gravity-viscosity relationships, which they do not, as I shall show, they would still be very far different from Burton's description, both in not being petroleum oils and in not being substantially saturated products.

Q. So in fitting the shale lubricants to the Burton product, Dr. McKee mis-stated the facts about the Burton product and then fitted the references on?

[fol. 999] —. The statement which plaintiff's expert made about the Burton patent was incorrect, and therefore the similarity which he concluded therefrom was also incorrect.

Q. In this connection, plaintiff's expert also quoted some statements of Lyder, at the Catlin Shale Oil Plant, as to the difficulty of producing high viscosity oils from shale. He said, also, that he was sure there was some data available, showing the gravity, viscosity relationship prior to 1913, but did not comply with the Master's suggestion that if he found them he might produce them as exhibits.

What information can you give as to the viscosity gravity relationships of shale oil lubricants?

A. I happened to have in my laboratory two shale oil lubricants obtained from this same Catlin plant, at which Lyder was working, and I had determinations made of their gravities and viscosities, and found that both of them had gravities between 25 and 26 Baume and viscosities of 237 and 358 respectively. This places the lubricants a little bit above the other prior art lubricants, one coming right beside the upper, solid black square on the chart, and the other coming above the imaginary giant oil of Aisinman, which is on the 25 gravity line, just above 350 viscosity.

I don't know whether or not those are typical, but those are the only data I have, and are interesting as coming from the plant in which Mr. Lyder was working, from whom plaintiff's expert quoted.

Q. When you said you had these, they were in your laboratory at the time plaintiff's expert was testifying—is that true?

A. Yes, sir.

Q. You had obtained them before that?

A. Yes, we obtained them to make some tests as to their lubricating value.

It is also true that shale oil lubricants certainly contain little if any of the peculiar saturated heavy gravity materials characteristic of Burton's pressure tar, and I should expect them to fall among the other prior art oils, as these particular ones do.

Q. Plaintiff's expert, in the course of his testimony, also said that the process of the twine oil patent, No. 1,167,884, was identical with the process of the Burton re-running patent, No. 1105961—what have you to say on this point?

A. The two processes are not identical, and both of them make perfectly clear exactly what they do.

The process of making the twine oil product, consists in cracking under pressure, a stock similar to gas oil, until somethink like 50 per cent. of distillate has been taken off, withdrawing the tar, distilling off at atmospheric pressure the lighter constituents of this tar, and thus making the twine oil distillate.

The re-run patent is a proces patent, which involves the steps of separating this twine oil product, and then of re-cracking this material, which Burton found was a suitably clean stock, which could be further cracked at temperatures and pressures prevailing in his process.

In other words, making the twine oil product included only two of the three steps of the re-run patent process.

Q. The re-run process has a third step, which is not in the twine oil patent.

A. That is correct.

The Master: The twine oil patent expressly states the prior art processes used in producing the twine oil—that is, No. 1049667, and No. 1,105,961.

The Witness: Yes, sir.

Q. Now, please state whether the basic idea underlying the twine oil patent and the re-run patent could have been arrived at by two separate men?

A. Yes, it is certainly entirely possible that the basic contributions involved in these two patents could have been arrived at by two different men—that is, one man might have taken the pressure tar and examined it, and distilled it, and found that he obtained a product which was unique [fol. 1000] in its content of hydrocarbon, and in its gravity-viscosity relationships.

A different man might have taken this product and found, by experiment—because it certainly could not be predicted—that this oil was a clean cracking stock, which could be cracked under Burton's temperatures and pressures to get a good yield of gasoline without developing hot bottoms.

Mr. Wiles: Now, your Honor, this chart from which the witness has been testifying, is offered as defendants' "Exhibit No. 417," and we have small photostatic copies of it that we can supply to counsel.

The said chart, so offered, was admitted in evidence, was marked "Exhibit No. 417," and the same is herewith returned.

Q. Now, what did Ellis contribute to the art of making motor fuel, in his treating patent No. 1318061?

A. So far as I can find Ellis was the first man to show that ordinary cracked gasoline could be successfully refined with a relatively small amount of dilute acid, in contrast to the strong acid which was ordinarily used in the petroleum industry.

In fact, Ellis' treat- for the cracked gasoline was distinctly loss severe than that ordinarily used for refining straight run gasoline, in spite of the fact that any person looking at the product, with its poor color, undesirable odor, and high content of unsaturated compounds, would naturally jump to the conclusion, as many in the prior art did, that a large amount of stronger acid would, if anything, be required to make this into a satisfactory marketable product.

Ellis, however, recommended the use of relatively small quantities of 40 to 60 per cent. acid, followed, if necessary by a small amount of slightly stronger acid. By this means he found it possible to make a fairly heavy gravity and relatively high end point gasoline, which normally carries a good bit of gum forming constituents and a bad color, into a satisfactory gasoline with a loss of only one or two per

cent., and leave in it most of the olefins, which it was previously considered had to be removed in order to make a satisfactory gasoline.

I don't mean to indicate that Ellis was the first one who made a gum free gasoline from a cracked distillate, because he was not. They had previously done that by either one of two methods. It was done, in the first place, by taking a very low end point product, just skimming the cream, so to speak, from the gasoline, which, since the gum forming constituents, as plaintiff's expert has stated, tend to concentrate in the heavy end of the gasoline, can be largely eliminated by the simple expedient of making a very light naphtha, rather than an ordinary motor gasoline, with a fairly high end point, and fairly heavy gravity.

The other scheme which has also been used, is to treat these gasolines so severely, with so much strong acid as to remove all the olefins and give a substantially saturated gasoline, like ordinary straight run gasoline in most respects. Both of these methods, however, involve the loss of from twenty to forty per cent., as a rule, of the original cracked gasoline, and Ellis' big contribution was in showing that it was possible to get a gasoline which was low in gravity and relatively high in end point, and yet free from gum forming constituents, without having to treat so heavily as to remove the olefins. In other words, he only got a loss of 1 or 2 per cent. by his process.

[fol. 1001] Q. Now, the Master asked plaintiff's expert, on page 970 of the Record, what an experienced refinery chemist would do if he had been asked, in 1913, to prepare a substantially gum free gasoline from unrefined cracked distillate.

As a matter of fact, was the knowledge of the industry sufficiently advanced, at that time, to make such a question as that at all probable?

A. No, it was not. Ellis was really almost ten years ahead of his time, in calling specific attention to these gums forming constituents, which he refers to as resinifying olefins.

The common objections, in that day, to cracked gasoline, were to its odor, its bad color, and, specifically, to its high content of unsaturateds, which was blamed for its lack of stability and its general difference from ordinary saturated gasoline.

Before Ellis no one ever realized that most of the olefins were really quite desirable constituents of gasoline. They considered that all of the difference between cracked gasoline and straight run gasoline, were due to 20 per cent. (or more, in some cases), of olefinic constituents, and the biggest step which Ellis had to make was to distinguish between the two—in other words, to ask himself the question whether he could remove the gum-forming constituents from cracked gasoline, and still leave in the olefins, it being so universally believed that the two were cause and effect, together with the bad odor and the bad color.

Q. Now, I believe you have already shown, have you not, the importance of the presence of gum?

A. Yes, I have already pointed out that the absence of gum forming constituents from gasoline is not an absolutely vital requirement, although it is one upon which, within the last few years, we have begun to lay more stress. As I have already said, the Government has, even today, no specifications for motor gasoline, to cover this point, because by and large, a reasonable amount of gum forming material doesn't do much harm, unless the gasoline is stored for a considerable time, or for a shorter time at elevated temperatures, in which case, it may form gum and may cause trouble in time.

Q. So that a man can run along quite a while, as the industry did, and not pay much attention to this gum question?

A. Yes.

Q. And Ellis was the first man who concentrated his mind on gum, at all, as far as you know?

A. Yes.

Q. Now, apart from the fact that nobody would, in 1913, ask the question that the Master asked, at all, suppose that someone had put such a question, what would the experienced refinery chemist naturally do?

A. He would naturally follow the common practice of the industry, which was to use strong acid—that is, generally 66 degree Baume, or 93 per cent sulphuric acid.

As Ellis points out in the beginning of this very patent, it was even the custom to first give the oil a light treat with this strong acid, to remove the water, so as not to interfere with the efficacy of the treatment by diluting the acid later added, since they considered made it almost useless to treat

with acid, which was much more dilute than the 66° acid commonly used in the petroleum industry.

Q. Those practices were common, were they?

A. Yes, they were, and are yet, to some extent. As a matter of fact, not only was the prior art very firm in its conviction that dilute acid was substantially ineffective as [fol. 1002] a refining agent for petroleum products, but it was many years after Ellis before even the larger companies realized the advantages of his method of treatment. Even since I have been with our Company, we have been tending more in the direction of Ellis. A few years ago, it is my understanding, that very few of the refiners were using Ellis' scheme, although it really is better than the use of the strong acid, so firmly was it fixed in their minds that the strong acid was the material to use in treating, and that this cracked gasoline, being a harder material to refine, would require larger amounts, and, if possible, stronger acid than that used on the ordinary straight run products.

Of course, if someone had told a chemist that there was a distinction between gum forming constituents and olefins, then he might have been led to try weak acid, which would not attack the olefins. But he was filled up with this idea that the olefins were responsible for the trouble, and that you must remove the olefins, and he naturally, therefore, went to strong acids, which were the recognized method for removing olefins.

The Master: To what extent is the Ellis process being used?

The Witness: To the best of my knowledge it is now used by practically all of the primary defendants—and I presume very largely by the secondary defendants—that is, the method of treating by means of a relatively small amount of dilute acid.

The Master: Is that method of treating with strong acids, to your knowledge, used anywhere?

The Witness: Yes, it is still used by some of the smaller refiners. I have heard, within two years, of some of the small refiners doing their treating with strong acids. Even today the advantages are not fully appreciated. It seems to the refiner as being an illogical, rather than a logical change from ordinary practice.

Q. Well, there is a nuisance about it in the kind of pipes and tanks you have to use, isn't there?

A. Yes, that is true. The dilute acid is more corrosive than the strong acid, contrary to what might be supposed. You have to take particular care in the design of your plant, when using the more dilute acid.

Q. In ordinary straight run gasoline what was there that was removed by the treating?

A. In ordinary straight run gasoline the main materials [fol. 1003] removed by the treating were the olefins, with a trace of asphaltic contamination which might have gotten in through dirty lines or contamination with a little crude.

Q. When cracked gasoline came in what was the assumption as to the acid that had to be used on it based on crude practice?

A. The natural assumption was that the acid would have to be increased from an amount sufficient to care for one or two per cent. of olefins as in straight run gasoline, to fifteen or twenty per cent which represents the olefine content of the cracked gasoline, if you were to obtain an equally satisfactory product.

Of course, some have tried to market it with practically no refining, thinking they could not afford the large losses involved in such refining.

The Master: What was the difference in the losses by the Ellis process and the strong acid process?

The Witness: Of course that depends on the amount of the acid. If you used amounts such as were fairly typical in the prior art, or if you started out on the basis of treating to remove the olefins, which was the natural assumption, you would lose fifteen to twenty per cent instead of one or two per cent.

The Master: That worked into dollars and cents in what degree?

The Witness: Well, that would of course enormously add to the refining cost, both on account of the greater consumption of acid and the fact that these light products which we had produced at considerable expense by cracking we would lose, and throw away into fuel oil or asphalt practically fifteen or twenty per cent of our product, which would be a very serious, and undesirable loss.

Q. Plaintiff's expert talked of the olefins as being responsible for gum formation; is this correct?

A. I do not think that we are entirely certain of that. It may or it may not be correct. That theory that the gum forming constituents were diolefins, rather than olefins, is a theory which has been developed by work done during the last ten or eleven years, partly to account for this very fact that we have now found, and first shown by Ellis, that you can remove the gum-forming constituents and leave in the olefins. That shows that the gum-forming constituents are not the ordinary, simple type of olefins, and it has been postulated, with some supporting evidence, that they are really diolefins, that is, chain compounds with two double bonds in the chain at some point.

This, as I say, is quite possibly true and is the general theory today, but our theories have had to change as time went on an- even that may not be considered true a few years from now.

However, as plaintiff's expert, when asked on pages 933 to 943 to cite any such distinction prior to 1913 between olefins on the one hand and gum-forming constituents or olefins, on the other.

[fol. 1004] The chemistry of 1912 and 1913 divided all petroleum hydrocarbons into paraffins and olefins; that is the saturated and unsaturated with respect to sulphuric acid. Since then we have split up the saturated into two different types of compounds, paraffins and naphthenes, and we have also split up the unsaturateds into two types of compounds, olefins and diolefins, ten or fifteen years from now we will undoubtedly have split them further down and be able to explain a little more definitely what some of the exact compounds are in our oils.

Q. Can you cite any references tending to show the general belief of the industry with regard to the undesirability of the olefins and the amount and strength of sulphuric acid required to remove them?

A. Yes, it is not necessary to go out of the list of exhibits cited by plaintiff's expert mainly cited against this very patent, to bring out the common belief of which I have spoken.

Thus, I read from page 1700 of the book of exhibits, from Barbet who states in two lines near the middle of the page, referring to his cracked products:

"These products can be treated with sulphuric acid for destroying olefins and thiophenes," and so forth.

You notice the use of the sulphuric acid was for the purpose of destroying the olefins, not of removing something and leaving in the olefins.

Q. He mentions thiofenes. To avoid any lack of understanding for any of us, those are sulphur compounds, are they?

A. Those are a certain class of sulphur compounds, yes.

Also, on page 4199 of the book of exhibits, Mills in Exhibit 245, which is a book on destructive distillation, says with reference to the use of sulphuric acid, in this case on shale oil, not on petroleum:

"This latter liquid (that is sulphuric acid) has but little action on the fatty hydrides proper (that is Mills' term for paraffin hydrocarbons or saturated hydrocarbons) but on hydrides containing less hydrogen (namely the olefins) it acted powerfully, resinifying and polymerizing them as it does turpentine. Weak acid sometimes used at this stage is comparatively inefficient for the purpose."

It will be noted that this reference to the use of weak acid is not on refining petroleum but on shale oil, and the statement in regard to it is that it is comparatively inefficient for the purpose of the olefins.

In the last paragraph of page 1508 of the book of exhibits, Armstrong and Miller, state with regard to use of acid of about 76 per cent strength:

"By its action the unsaturated hydrocarbons are, for the most part, converted into compounds which are not distillable in steam. In order to effect as complete a conversion as possible, it is desirable when the action appears to be at an end, to separate the acid from the oil and treat the latter with stronger acid."

Q. In other words, in that reference they finish up with strong acid to complete the conversion of the olefins?

A. Yes, and they say definitely even the 76 per cent acid which, of course, is stronger than Ellis' preferred acid, is sufficient for the most part to convert the unsaturated hydrocarbons into materials which can be removed. The materials which they are treating are the distillates from oil gas tar.

These same authors say on page 1519 that the olefins "If free from benzene are entirely converted into polymerides and soluble bodies on treatment with sulphuric acid—"

Q. When they say they are entirely converted into polymerides, does that mean the same thing that you spoke of as polymerize or polymerization the other day?

A. Yes. That is one of the actions of sulphuric acid.

[fol. 1005] Q. In other words, you are calling attention particularly to the statement that they are entirely converted?

A. Yes.

Dewar & Redwood say on page 1385, with regard to kerosene made by cracking:

"It is asserted that such oil is inferior in light-giving power and more readily acquires a dark color on keeping. It is certain that the smell of the cracked oil is stronger and that in the chemical treatment a much larger quantity of sulphuric acid is required."

That is exactly the trend that I have been speaking of, that the natural thing to do is to treat these inferior products more heavily rather than less heavily.

On page 1489 of the Book of Exhibits Aisinman discusses precisely the same question of cracked kerosene and says:

"The illuminating oils obtained by decomposition distinguish themselves by ready oxidizability, and for their chemical purification and refining they require a higher percentage of high grade sulphuric acid. The refining losses are rather high since the hydrocarbons react intensively with the sulphuric acid. By means of pure 66-degree sulphuric acid alone it is not possible to produce water-light or slightly yellowish petroleum, but more success is achieved by the use of oleum of about ten per cent."

Q. What does oleum of about ten per cent mean?

A. Oleum of about ten per cent means that about ten per cent of sulphuric acid anhydride has been dissolved in sulphuric acid making it still stronger, in the neighborhood of one hundred per cent sulphuric acid.

In other words, what Aisinman states clearly, (and Aisinman is talking about refining kerosene in the ordinary practical petroleum industry)—is that these illuminating oils obtained by cracking are readily oxidizeable and require

larger amounts preferably of stronger acid in order to treat them and make a satisfactory product. In fact, he says you cannot make a satisfactory product even with ordinary 93 per cent acid.

Dr. Krey, who did a good deal of work in this field and developed the Riebeck process, apparently considered it was necessary to remove all of these olefins in order to make a marketable kerosene, because he states, on page 4229, that his total loss is about forty per cent of the gas oil used in his cracking process, and that of this loss, ten [fol. 1006] per cent is shown by a reference on the previous page, to consist of gas, water and coke, which is lost in the cracking process.

That gives him ninety per cent of liquid products, and since his total loss is forty per cent, that leaves him sixty per cent out of this ninety as finished, refined products; meaning a loss in refining of about one-third of the liquid products gotten from the cracking operation.

This checks with his statement on the next page that of the distillate produced about one-third consists of unsaturated hydrocarbons.

This general attitude of the prior art toward ordinary cracked products might be expanded quite a bit by a reference to other exhibits which do not happen to be cited in the case, but this seems to me to be quite adequate to show that instead of Ellis' expedient being obvious, it meant going exactly in the opposite direction from that in which the prior art clearly pointed; namely, toward the use of larger amounts of stronger acid in refining to make marketable products out of cracked products.

Q. Now, do any of the scientific papers of the plaintiff's expert indicate when he himself first considered it obvious to treat cracked products very lightly so as to remove the gum-forming constituents and leave in most of the olefins?

A. It was evidently not until recently that plaintiff's expert considered this quite obvious, because in his paper in Chemical Age for January, 1922, near the end of the first page, the statement is made:

"In New York it is not uncommon to have put in one's car a gasoline carrying 20 per cent of unsaturated constituents, which means that such gasoline on standing and in contact with air discolors and separates after a time a small amount of brown tar-like deposit."

In other words, although by this time practically all of the refiners realized the advantage of the Ellis process and that it was possible to remove the gum-forming constituents and leave in the olefins, plaintiff's expert still believed that the fact that olefins were present necessarily meant that the gasoline would form gum on standing.

Also, in the early part of his testimony page 536, plaintiff's expert makes the statement:

"Our ordinary method of purifying a mixture containing paraffin and other hydrocarbons is to change the other hydrocarbons by severe chemical treatment, which chemical. [fol. 1007] treatment, even though severe, is essentially without effect upon the paraffin hydrocarbons."

Q. And that was true of the general ordinary method of purifying such a mixture some years ago, was it not?

A. Certainly before the time of Ellis.

Q. The master also asked in a question on page 970 if the refinery chemist would naturally have turned to the Armstrong treating method for the removal of the gum-forming constituents, and plaintiff's expert said that he would. Please state your opinion on this point.

A. I do not see any reason why he should, because Armstrong and Miller were not in any sense a part of the petroleum refining art. Armstrong and Miller describe a laboratory method of purifying the aromatic hydrocarbons in oil gas tar. There are practically no paraffins present, and they were interested in getting the impurities, particularly the olefins, away from the aromatic hydrocarbons.

For that purpose it was an obvious expedient to use comparatively dilute acid, because it was known that strong acid reacted with the aromatic hydrocarbons which they were trying to purify, but that was a special expedient to meet a special situation in a diffeernt industry. It seem- to be nothing that a petroleum refiner would have obviously used from the fact that dilute sulphuric acid might have been used for some other quite different purpose, in some other quite different industry.

Q. You stated that the strong sulphuric acid was known to react. Would that have destroyed the aromatics, such as benzol, which were what they really wanted?

A. Yes, it would have reacted with them to a very considerable extent.

Q. And the reaction would have spoiled them?

A. The reaction would have spoiled them for the desired purpose. That was a cleancut problem of getting something that would remove olefins and other impurities from aromatic hydrocarbons, which forbade their using as strong acids as were commonly and customarily used in the petroleum industry for quite different purposes, and bore no relation to Ellis' problem.

If this problem had been put up to a refinery chemist, as I have already stated, he would naturally use the expedi-[fol. 1008] ents which were common in the petroleum refining industry, namely, the use of relatively strong acid, and the natural step to take would be to use more and stronger acid because of the less saturated character of his product.

Q. Suppose, however, for some reason a refinery chemist did try out the Armstrong process on Ellis' cracked motor spirit, would he accomplish the disclosed result of the Ellis process?

A. No, he would not, because Armstrong and Miller specifically say that by their first treatment, as I pointed out, they largely polymerize the olefins, and by their second treatment with a stronger acid, about 85 per cent, they state that it is to effect as complete a polymerization as possible; and I also pointed out that they stated on page 1519, that the olefins, "if free from benzene, are entirely converted into polymerides and soluble bodies on treatment with sulphuric acid." In other words, if you did use Armstrong and Miller's process and followed their directions you would treat with so much of this acid as to remove substantially all of the olefins.

Q. And that would give you about twenty per cent refining loss, or something like that?

A. Something of that sort, yes.

Q. Plaintiff's expert stated on cross-examination at page 967 that the unpleasant odor which Armstrong and Miller ascribed to their product was evidence that they did not remove substantially all of the olefins. Is that a correct interpretation of their statement?

A. No, it is not. In the first place, the quotation is not accurate. The statement which Armstrong and Miller make on page 1509 of the Book of Exhibits refers to volatile

products of the action of the acid, not volatile products and the action of the acid. These volatile products of the action of the acid are unquestionably sulphur dioxide and other sulphur compounds made by reaction between the acid and the oil, certainly no one would consider that olefins are a product of the action of the acid.

In fact, the acid is to destroy the olefins, not to form the olefins. Furthermore, these olefins do not have bad odors, as plaintiff's expert implies. For example, the refined gasoline, which I showed a short time ago has around twenty per cent of olefins, and yet a very satisfactory odor and nothing like what Armstrong and Miller were referring to, the unpleasant strong odors which are produced by acid-treating oils.

[fol. 1009] Q. What reference did plaintiff's expert cite against the Ellis treating patent?

A. He cited Aisinman, Exhibit No. 118; Redwood, Exhibit No. 116; Mills, Exhibit No. 245; Vittenet, Exhibit No. 117; Armstrong and Miller, Exhibit No. 119; Letny, Exhibit No. 158, and Allen, "Exhibit No. 113."

Of these, Allen merely describes certain of the products, and does not discuss, so far as I have been able to find, any methods of treatment. So that it is really a reference to the product patent which we will consider later, rather than to the treating patent.

Q. Do any of these references show the treatment of a cracked motor fuel at all similar to that of Ellis?

A. No, none of them do.

Q. Which of the six references which describe treating processes can properly be considered as referring to the petroleum industry?

A. Aisinman is the only one which clearly refers to the petroleum industry, and he refers to the refining of cracked kerosene, which involves problems somewhat similar, although by no means identical, with those in refining cracked gasoline. As I have pointed out, Aisinman specifies the use of large amounts of 93 per cent acid, or preferably of oleum. So it is just the opposite of being a reference for Ellis: It is a clear showing of how far Ellis departed from the prior art in his new treating process.

Q. What do the other references relate to?

A. Only one of them clearly refers to anything in the petroleum industry, and that is Aisinman. Vittinet probably referred to shale oil, but it is probably shale, in any case, but if it was petroleum, it was not a cracked product such as was the case with Aisinman. Redwood and Mills discuss the refining of shale oil, while Armstrong and Miller and Letny discuss the aromatic distillates from oil gas tar which are entirely different from cracked petroleum distillates, such as Ellis was dealing with.

Q. And how heavy is Vittenet's treat?

A. Vittinet uses 48 pounds per barrel of 78 per cent acid, plus 48 pounds per barrel of sodium bisulphate, to be followed if desired by a second treatment of similar kind. I do not know just how effective the sodium bisulphate would be, but he says that in place of this treatment, as an apparent equivalent thereto, you may use 48 pounds of 93 per [fol. 1010] cent acid plus a little potassium dichromate. Either of these treats is, of course, very large in amount, much larger than are ever considered for use on light petroleum distillates, and the last one is an extremely severe treat with 48 pounds of 93 per cent acid. Even the first with 48 pounds per barrel of 78 per cent sulphuric acid would remove a very large proportion of the olefins from any cracked product on which it was used. Of course, Vittenet is not talking about a cracked product but about purifying crude petroleum, improving its odor, and also a method of treatment for other products.

Q. Do any of the references cited disclose treating with forty to sixty per cent acid as specifically recommended by Ellis?

A. No, none of them disclose anything in that range. In fact, the only one that shows less than 75 per cent is Letny, who uses 64 per cent acid, and he also, like Armstrong and Miller, is treating a highly aromatic distillate from oil gas tar, and uses weak acid for the particular purpose of avoiding action on the aromatic hydrocarbons which he wants to separate.

Q. Which one of these six references do you think comes the closest to treating the Ellis product, and what treatment does that reference disclose?

A. I have already said the one which comes nearest is that of Aisinman, who treats a cracked kerosene from pe-

troleum, and recommends large amounts of 93 per cent acid, or preferably still stronger acid.

Q. What products of those cited as being acid treated would you say approached most closely to Ellis cracked motor fuel, after this Aisinman cracked petroleum?

A. There is really nothing else which at all closely resembles the product that Ellis is having to treat. Possibly the nearest of any of the other ones is the shale naphtha, which is cited in Redwood's book, Exhibit No. 116, where he describes treating with acid of unspecified strength, presumably 93 per cent, because that is ordinarily what is assumed when nothing else is specified. But this, it will be noted, is treating a naphtha made from a shale oil, which has previously been treated with a large amount of sulphuric acid, in which process 26 to 30 per cent, Dewar & Redwood say, has been treated out. In other words, the loss on treating the once run shale oil amounts to 26 to 30 per cent in the process of acid and alkali treatment, (mostly [fol. 1010½] in the acid treatment) and even after taking out this large proportion, which is to a considerable extent olefins, the shale naphtha is apparently still treated with strong acid, although only a small amount.

In this shale art they take the crude shale oil, put that in the still and re-run it to coke, taking the distillate overhead, making a "once run shale oil" which is more saturated and a more desirable product than the original shale oil, as I have already pointed out.

In this Redwood process they treat it with acid and then with alkali, using either about 79 per cent acid or acid tar left from a strong acid treatment of some other products. By those treatments they remove from 26 to 30 per cent, as Dewar and Redwood state on Page 1430. Having treated, the once run oil in this process and taken out 26 to 30 per cent of it, the oil is fractionated; that is, distilled, to separate the lighter products from the heavier products. And then they take the distillates, which were already treated when they were in the once run mix, and treat them a second time with acid of unspecified strength, which would presumably mean the ordinary 93 per cent acid. [fol. 1011] Redwood and Mills treat once run shale oil with acid varying from 77 to 79 per cent in strength. Mills specifically states that this is to resinify and polymerize

the olefins; and Redwood, as I have already pointed out, states a loss in this and the alkali treatment of 26 to 30 per cent, which shows how greatly different their process is from that of Ellis, even if they were in the same industries which they are not.

Vittinet, as I have already stated, uses 48 pounds of 78 per cent acid, followed by 48 pounds per barrel of sodium bisulphate, followed by a second treatment if desired, or alternatively using 48 pounds of 93 per cent acid together with a small amount of postassium dichromate. These are entirely different types of treats from what Ellis is doing, and would not accomplish Ellis' purpose—namely, removing the gum-forming constituents and leaving in most of the olefins.

Armstrong and Miller and Letny, as I have said, do use somewhat dilute acid in quite a different industry, namely, in the laboratory refining and examination of the distillates from oil gas tar, which contain large proportions of aromatic hydrocarbons and very little paraffin hydrocarbons. Even they speak of their refining as being to effect as complete a polymerization as possible, or to complete the conversion and polymerization of unsaturated compounds. They want to separate out pure benzol, and that is the reason why they go to dilute acid, which does not attack benzol; but of course requires larger amounts in order to get rid of the olefins which they wish to destroy.

Q. Do any of these have any indication of either the possibility or the desirability of removing gum-forming constituents by a lighter treat of dilute acid and of leaving in the major part of the olefins?

A. No, none of them even hint at the possibility or the desirability of such an operation.

Q. Ellis, however, discovered apparently both desirability and possibility?

A. Yes.

Mr. Wiles: It is a little after four, if the Court please, and I would like to adjourn before taking up the Ellis product patents.

—. Armstrong and Miller and Letney, by the use of moderately dilute acid in the treating of benzol, were trying to

remove the olefins and other unsaturated compounds from benzene or benzol, as I have generally called it in this case. That was the reason they employed the acid which was more dilute than that ordinarily used by the petroleum industry, but it is very apparent particularly from Armstrong and Miller's citation that they desired to remove every trace of the olefins even with this more dilute acid by using a sufficient quantity.

Armstrong and Miller state very clearly that they did remove all of the olefins and this is indicated also by the description of the product which Letney got after treating with his acid.

[fol. 1012] In other words, they were doing again just what Ellis wanted to avoid, that is, removing all of the olefins from the product in addition to the fact that they were not treating a petroleum product but were treating a highly aromatic distillate from oil gas tar, which involves quite different problems.

Q. Now, I believe you said yesterday also that the oleum recommended by Aisinman for treating cracked kerosene was about one hundred per cent sulphuric acid. Please state in a little more detail just what oleum is.

A. Oleum as a class represents sulphuric acid which contains some free sulphuric anhydride or SO_3. The methods of expressing concentration of sulphuric acid are a little peculiar because of the fact that the acid itself contains a certain amount of combined water and you can put in the anhydride of that acid without the corresponding water and get an acid which is really stronger than one hundred per cent acid, because it contains free SO_3 to which you can add more water, and after the addition of water have acid that is one hundred per cent strong. So, from that standpoint, it is really stronger than one hundred per cent sulphuric acid.

The term "oleum" is used to refer to these acids which contain more SO_3 than corresponds to one hundred per cent acid.

Aisinman speaks in one place of ten per cent oleum and at another place of ten degree oleum. I am not certain exactly what ten degree oleum means, and even the term ten per cent oleum is not a term which is very generally used in this country, and I cannot be sure exactly what it meant

in 1900 when he was writing; but presumably it referred to an acid containing ten per cent of free SO_3, or sulphuric anhydride.

Ten per cent SO_3 would require a little over two per cent water to make sulphuric acid from it, and it might therefore be referred to as a little stronger one hundred and two per cent sulphuric acid.

Q. Now, I wish you would state the nature of the products described and claimed in the Ellis product patent No. 1,318,060.

A. Ellis describes in this patent a product made by cracking oil to get a heavy gravity, relatively high end point gasoline, which has been refined to remove the gum-forming constituents and to give it a good color and odor without losing the olefins.

In other words, it is a product which still contains a large [fol. 1013] percentage of olefins and yet has been so treated as to remove the gum-forming constituents.

In order to indicate the presence of these olefins and show that his product was not merely a product which had been refined by treating out all of the olefins he specifies the iodine number of his product as being ordinarily around 75 and always higher than 30. This, of course, compares with iodine numbers characteristically around five for a straight run gasoline, or for a cracked gasoline which has been heavily treated for the removal of the unsaturated compounds. Ellis unfortunately does not say exactly what test he used in determining his iodine numbers, but it is presumably the Hubl test, because this is the only strict iodine number and it is the old standard test.

The more recently developed Hanus and Wijs tests employ mixtures of bromine and iodine or chlorine and iodine, and while the results are usually calculated in terms of iodine, actually the material which adds to the double bonds is probably very largely the more active materials, chlorine and bromine; so that in a strict sense, I should say the Hubl test was the only one that could properly be called an iodine number and is probably the one that Ellis was using.

I want to make clear that Ellis did not indicate for a moment that his unrefined product was new; that is, his product of cracking, but rather that it contained the gum-forming constituents and had the characteristic bad order. It

was only after it was refined in a particular manner that he got his new product.

Furthermore, I also want to make clear that it is not merely a gum free cracked gasoline that Ellis is describing, because as I pointed out yesterday, other methods were known for making gum free gasoline from cracked products or from highly unsaturated products, one being to treat so heavily as to remove all of the olefins, which gave a big loss. Another method was to use only the light ends of the gasoline, material boiling up to 250 or 300, which is ordinarily rather free from gum-forming constituents.

The problem does not begin until we have a considerable amount of the heavier end of gasoline, boiling between 300 and 437 or even higher. Those are the constituents which make the trouble.

A. To be really the equivalent of that of Ellis the product must not only be a suitable motor fuel made by cracking petroleum, but it must have a good color, a good odor and be substantially free from gum-forming constituents, because those are the things which he was definitely aiming for, and definitely describes his product as possessing.

In addition to that, it must contain a considerable amount of olefins as indicated by its iodine number, and it must be a relatively heavy gravity type of gasoline.

The latter two limitations, you understand, are not merely arbitrary limitations to make the problem hard, but they are the intrinsic guarantee that Ellis' freedom from gum was not obtained by either of the two wasteful expedients of eliminating all the heavy ends or of treating out all of the olefins by the use of strong acid.

[fol. 1014] No such product was described in any of the references. Most of the products had not the slightest indication that they were free from gum, and from their method of preparation, we would be satisfied that they would not be free from gum and not have a good color.

The Master: What I understand you to say, Mr. Wilson, is that you cannot obtain the compound or product of that claim without employing the Ellis process?

The Witness: I have not so indicated, your Honor. In fact, we do know other methods by which such a product can be obtained. That is, it can be obtained by passing the

cracked vapors through a bed of Fuller's earth in accordance with a patent that issued a few years ago, which does eliminate these gum-forming constituents and leave the other materials. That was subsequent to Ellis, of course.

Q. The essence of the thing is that it shall be a heavy gasoline, that is, a gasoline like modern motor fuel, such as is described in the specifications; high in olefins and still free from gum?

A. That was not found in the prior art. As I said, most of the material described almost certainly contained large quantities of gum forming constituents. In the few cases where it does appear that they were probably gum free, this freedom from gum was accomplished either by one or the other of the two expedients which I mentioned; that is, by simply taking a low boiling cut in the one case, or by treating out all the olefins.

I will discuss them individually as we come to them.

Plaintiff's expert cited twelve references as follows:

1. Riebeck, Exhibit 97.
2. Engler and Schneider, Exhibit 98.
3. Walker, Exhibit 111.
4. Thorpe and Young in their articles Exhibits 94, 95 and 96.
5. Williams, Exhibit 114.
6. Cabot, Exhibit 112.
7. Armstrong and Miller, Exhibit 119.
8. Boynton, Exhibit 109.
9. Bassett, Exhibit 110.
10. Spears, Exhibit 108.
11. Allen, Exhibit 113.
12. Allen, Exhibit 115.

In a very general way the first four references give a cracked product which contains olefins and gum-forming constituents, and therefore, in a general way would be similar to Ellis' starting material, although of these first four only one started with petroleum; that is Walker; Riebeck starting mainly with oil gas tars or brown coal tar oils; Engler and Schneider using heavy lignite oils, and Thorpe and Young cracking mainly paraffin wax; but, in a general way, the distillates which they secured presented similar refining problems.

Q. In listing those you followed the same practice that you followed in your testimony, of listing as one reference, a single process, even though it was described in several articles?

A. I think the only place where there is any combination is in the case of Thorpe & Young's disclosure, which is spread through three articles.

Q. You have treated them as a single reference?

A. Yes, sir.

Q. Now, do any of these four references—that is, Riebeck, Engler and Schneider, Walker, and Thorpe & Young, describe a method of treating this cracked product to get an unsaturated gum free product, similar to that which Ellis obtains by his dilute acid, and claims as new?

A. No, none of them disclosed any such treatment. Considering these four individually, neither the Riebeck nor Engler, nor Schneider, describe any treatment for their gasoline, or benzine portion, though Riebeck does refer to some entirely undisclosed kind of treatment for his kerosene.

[fol. 1015] Walker, the third of these references, treats with hypochlorous acid, which, as he clearly indicates, reacts with substantially all the olefins, and leaves behind a substantially saturated product.

In the Thorpe & Young articles there are two chemical treatments described, one of which is distilling from sodium, and the other is treating with bromine. The bromine treatment destroys the olefines adding to the double bonds, in the manner that I described some time ago.

It is specifically designed to react with all the olefins.

The other method of treatment is distilling from sodium. Thorpe & Young do not state the purpose of that treatment.

Plaintiff's expert said that it would unquestionably polymerize and remove the gum forming consti-uents, and when asked, on cross-examination, what basis he had for that, he mentioned the action of sodium or isoprene, a type of hydrocarbon used in synthesizing rubber.

I believe, however, that no one has ever proven the presence of any isoprene in cracked gasoline, and from the little that we know about the chemistry of these materials, we know that reasoning by analogy—especially when we do not have the same compounds—is very dangerous.

In order to see whether distilling from sodium would, or would not have the action of polymerizing the gum forming constituents in gasoline, I took a sample of pressure distillate made by one of defendant's cracking processes, and distilled it to recover the gasoline fraction. This gasoline fraction had a gum test of 33.8 milligrams.

There, really, is no point in giving the tenths of milligrams, so I prefer to call that 34 milligrams, in around figures. If you ran another test that might easily come out 38 or 28, because the test is hard to make exactly reproducible.

I then took the same pressure distillate and distilled it to make gasoline, but I had sodium in the flask during the distillation, and carried out the distillation very slowly, so as to give the sodium a chance to have the polymerizing action which plaintiff's expert claimed for it.

The product I got from that had 28 milligrams of gum. That was, as you see, a very slight apparent improvement, but really within the limit of error of the method. We would have to repeat it a good many times to determine whether there was any improvement, at all. But this would not be a satisfactory gasoline, since it would not meet our own specifications.

To get a comparison of this, I took this same pressure distillate and gave it a light treat with a small amount of dilute acid, as Ellis suggests, and then distilled off the gasoline cut, and in that I got four and a half milligrams—make that 5 milligrams of gums—(I don't want to give the impression that these tenths of milligrams mean anything) —which is about what our gasoline normally runs, and which we do consider a quite satisfactory gasoline.

In order to try out this point further, I took a cracked gasoline made by some high temperature tube cracking process, which originally had 149 milligrams of gum. Instead of distilling off from sodium, this time I tried refluxing for several hours, with sodium in the flask—that is, boiling up into a condenser, and allowing the condensate to drain back, to give it a maximum chance to get contact.

Then I filtered off the suspended matter from the sodium and determined the gum in the material which came through.

Any olefins which had been polymerized to gum by this action of the sodium, would have been removed by filtra-

[fol. 1016] tion, but the filtrate tested, in that case, 165 milligrams of gum, showing an apparent increase in the amount of gum forming constituents. That, also, as compared with 149 milligrams before treatment, may be within the limits of error of the method.

This material was then re-distilled from sodium, leaving a small residue in the flask, and the distillate showed 133 milligrams of gum, but part of that reduction was due to the fact that five per cent of the heavy ends remained in the flask.

Then I repeated this distillation without sodium, and got material with a slightly lower gum test than that, showing that in those cases, the sodium had no beneficial action, in that it seemed to have slightly increased the proportion of gum forming constituents.

It is just another example of the uncertainty of theoretical reasoning as to what ought to happen in this art, where we don't know our fundamental chemistry well enough to predict what ought to happen. But it certainly does appear that the sodium treatments of Thorpe & Young do not have any appreciable beneficial effect on gum, and do not accomplish anything like the same beneficial result that the treatment of Ellis did.

I may say, in this respect, that I was rather surprised that there was not more action in that direction. Reasoning from pure theory, one would expect a somewhat greater effect.

Q. And the action that plaintiff's expert predicted, simply wasn't there, at all?

A. No. I might say that the color of the distillate, when we distilled from sodium, was considerably better. Just to look at the color you would think there had been quite an improvement in the product. That may be the reason that Thorpe & Young distilled from sodium, mainly to improve the color of their product, but these gum forming constituents, whatever they may be, do not seem to be appreciably affected.

Q. Are you willing at the plaintiff's request, to repeat these runs in their presence?

A. Certainly. Of course, I cannot guarantee identical results from the gum test, because it has not been standardized to the same extent as tests for other properties.

The time is very difficult to control, and that makes it possible to get considerable variation.

Q. How about the lower fractions that Thorpe and Young made?

A. The lower fractions of Thorpe & Young may have been gum free as they came off. That is quite possible, although they probably had small amounts of gum. They would probably be suitable for motor fuel, but, of course that simply dodged the problem of Ellis, instead of solving it.

The Master: When you say lower fractions, you mean fractions below a certain figure?

The Witness: Yes, boiling below 250, or something of that kind.

Q. Now, did Plaintiff's expert, in his testimony, indicate that the gum containing cracked products, made by the first four references which he cited, might be made into a product similar to that claimed by Ellis?

A. Yes, he indicated that by applying a treatment like Ellis did, to those products, you could get a similar product, and, of course, there is no question but that, if you started with the same materials and applied the same treatment, you would get a similar product.

But, as I have already pointed out, this was not an obvious expedient, at all, in this art—that is, treating with dilute acid to remove the gum forming constituents, and leave the olefins, because the art thought that the two things were one and the same, and that the only thing to do, if you wanted to get a really stable and satisfactory product, was to give a heavy treatment of acid, which would not give Ellis' type of product.

[fol. 1017] The next three references, namely, Williams, Cabot and Armstrong & Miller, all describe liquid by-products of the gas making industry, that is, products which were cracked at a very high temperature.

All these products are similar, in containing a large percentage of aromatic hydrocarbons, and therefore, they are radically different from those of Ellis, which contained only the ordinary petroleum hydrocarbons, even though they were gum free.

However, if those products were made under conditions which would give any appreciable amount of olefins, to

resemble Ellis products, they, unquestionably, were far from being gum free.

Q. Now, assuming that all these products were made under such conditions that they did contain olefins, and also gum forming constituents, as well as aromatic hydrocarbons, were any treating methods described which would remove the gum forming constituents, and still leave the olefins?

A. No. Neither Williams nor Cabot describe any method of treatment, or purification, whatever, while Armstrong and Miller, as I have previously pointed out, specifically disclose the removal of all the olefins.

Q. And that is just what Ellis was trying to avoid, was it not?

A. Yes, sir.

Q. So that these three references not only do not have Ellis' starting material, but they either did not treat at all, or they treated just the way he didn't want to treat?

A. Yes.

The next two citations are quite peculiar. They both merely describe the mixtures of ordinary petroleum naphtha and of benzol. The only ground for possibly considering them as references was because plaintiff's expert in the earlier part of his testimony apparently thought that benzol had an iodine number. He later was asked to perform experiments, and admitted that it did not have an appreciable iodine number. Therefore these references have not the remotest resemblance to Ellis' product, being merely mixtures of benzol and straight run naphthas, neither of which has an appreciable idone number.

Q. And this question of the iodine number of benzol you have occasion to go over later in your testimony, have you not?

A. Yes, in another connection.

It is very difficult to see the basis of the citation of the Spears patent, Exhibit No. 108, against Ellis. As appears from the exhibit, Spears claims to make a good illuminating oil from benzine and tar, which he claims are caused to perfectly unite by heating the benzine and tar up to a [fol. 1018] high temperature without pressure. Of course, anyone skilled in the art knows that if you take any such mixture as benzine and tar and heat it up, the benzine

merely distills off long before you get to a cracking temperature or bring about any change. Then when you get the tar hot enough maybe some of its light ends will distill off, and if you keep on heating the tar will crack and eventually coke.

I suppose the reference might be considered as disclosing anyone of three things—either the illuminating oil which he claims to make, but cannot make, which is certainly nowhere near the same as Ellis; or the benzine which distills off at the start and does not have any appreciable iodine number, and is simply an ordinary straight run benzine; or the mixture of the benzine and the heavy cracked distillate that goes off from the tar near the end, which would contain the normal amount of gum-forming constituents and for which no refining procedure is suggested.

So none of the three possible interpretations of the reference give a product which is at all like Ellis'.

Allen in page 1405 of Exhibit 113 and page 1424 of Exhibit No. 115 tabulates some gravities and analytical data for two commercial gasolines, as he calls them, one being made from shale and one from petroleum, and two naphthas, also, one made from shale and one made from petroleum. Apparently, the products cited in the two tables differ very slightly, but inspection of their gravities in particular shows that they must be the same materials referred to in the two references.

With regard to the commercial petroleum products—that is, the gasoline from petroleum and the petroleum naphtha—it is highly improbable that either of them contained any appreciable amount of olefins, certainly not enough to come close to what Ellis is describing as his essential cracked products.

In other words, these are not described as cracked products in any sense, but simply commercial gasoline which Allen picked up and analyzed. Unfortunately, they do not give any iodine number, but they do give bromine numbers. The bromine numbers given on page 1405 are different from those given on page 1424 probably due to the fact that he used a different method in the latter analysis. If we should take these latter numbers by the latter method for [fol. 1019] bromine and multiply by 1.6, the ratio which

Dr. McKee said would convert bromine numbers into iodine numbers, we would get one value as high as 29.3, which is almost up to the lower limit of Ellis' product, but no-where near his actual preferred product or ordinary product which had an iodine number around 75. However, as I shall show, the use of any such conversion figure as 1.6 is highly unreliable for getting iodine numbers from bromine numbers.

Furthermore, these points are not particularly essential, because these references very clearly fail on the question of the gravity of the product; that is, in being a heavy gravity gasoline which has this difficult problem of removing the gum from the heavy ends. Instead of being heavy gravity gasolines, they are very light, the heaviest one from petroleum being 69 Baume. His shale naphthas do, of course, have iodine numbers in the general range specified by Ellis, because of their high content of olefinic hydrocarbons, but the two gasolines given have gravities in the 80's, and the two naphthas given have gravities of 65 and 69, respectively, on page 1424.

As I say, these are very clearly not the heavy gravity products. I do not know whether the shale naphthas were free from gum or not. Plaintiff's expert contended the fact that they were commercial products indicated that they were free from gum. I have seen many commercial products, particularly commercial shale gasoline, which was far from being free from gum-forming constituents.

On the other hand, when we refer to the description of the boiling points, of these products, on page 1426, we find that even the naphthas, which had the heaviest gravities—that is 65 and 69 gravity, respectively, which would be high test products judging gravity alone—he says that these products, petroleum spirit and shale naphtha of approximately the same gravities, distill mostly between 65 and 100 degrees centigrade, or between 149 and 212 degrees Fahrenheit.

In other words, they very probably were free from gum, by virtue of the mere fact of their low end point, but they in no way resemble Ellis' product and they do not have to meet Ellis' problem of removing gum from material boiling above about 300 degrees, because, as I say, the main portion of these boil from 149 to 212. While they are called

naphtha, they nowhere near resemble gasoline. And the other two products which are called gasoline are still lighter, having gravities, as I say, in the 80's.

[fol. 1020] So that none of these four products have the heavy gravity or the relatively high boiling portions which characterize the Ellis product, and which really constituted Ellis' problem, which he overcame. These products merely dodge this serious problem.

Q. And the suitability of such products for general motor use you discussed thoroughly in the early part of your testimony, where you were considering the requirements for gasoline, did you not?

A. Yes. The term "gasoline" was very frequently used in the early days to describe a product used for making gas in household machines. That is, they used products which were so volatile that in certain types of machines they would make a combustible gas when put into the gas-making machines.

Q. By evaporation?

A. By evaporation. These products are today referred to as gas-machine gasoline.

Q. They do not meet the definition of the American Society for testing materials or that given by the Supreme Court in the case you referred to as to what gasoline is?

A. No, and they are not satisfactory materials at all for running a car.

Q. Now, Mr. Wilson, there are in this record a considerable number of statements by plaintiff's expert which I believe are untrue in point of fact, and while I do not see their particular importance individually and cannot see just what use may be made of them, I want you to go over the actual facts on each of these separate points. I will call them to your attention one by one.

In attacking this same Ellis product patent that you have just been talking about, plaintiff's expert said on page 355 of the printed record that Riebeck in his patent No. 37,728, produced by his cracking process an easily mobile fluid of about .800 specific gravity. Just where and how is this product described?

A. This product is described on page 1290 of the book of exhibits in Exhibit No. 97, the fifth line from the bottom

of the page, as "a brown, very mobile fluid of .820 specific gravity at 17½ degrees centigrade."
[fol. 1021] The translation, however, contains an error in that the specific gravity should be .200 to .820 as reference to the original article shows.

The description goes on to say, "This decomposition product is absolutely equivalent to the crude illuminating oil of brown coal tar, and after it is treated (in some unspecified manner) it yields a water white illuminating oil of .815 to .820 specific gravity." * * * in other words, a rather heavy illuminating oil of about 40 degrees Baume.

Q. In the next line, on page 355, the plaintiff's expert said that Riebeck's starting material was the same as that of Ellis; is that true?

A. No, that is not true. Riebeck's starting material is a dark, heavy paraffin oil of 23.6 Baume, whereas Ellis starts with a kerosene of 42 Baume. Riebeck's material, as I have shown before, is a brown coal tar oil, whereas Ellis' is a relatively light petroleum product. That is the starting material for the cracking process.

Q. In the next line plaintiff's expert says that the processes of Ellis and Riebeck are the same; is that true?

A. No, it is not. Ellis discloses in this patent a tube cracking process at atmospheric pressure and probably quite a little higher temperature than Riebeck's. Whereas, Riebeck has a bulk pressure distillation process, involving a long time and the moderate temperatures characteristic of that type of process with the production of mainly kerosene.

The two processes are almost as different as two cracking processes could be.

Q. Now, plaintiff's expert further says: "Accordingly, we would expect the same character of the products". Is that true?

A. In view of the fact that they are starting with such radically different materials, (it would be hard to find two materials containing largely hydrocarbons which are much more different than their two starting materials) and are using such radically different processes, we certainly would not expect the resultant products to be anything alike or similar.

As a matter of fact, we do not need to speculate on this point because Ellis clearly describes his product as a rather heavy gravity motor fuel; whereas, Riebeck's product is clearly described as being a heavy kerosene about 40 degrees Baume in this particular reference.

[fol. 1022] In other words, Riebeck's product is closer to Ellis starting material than it is to his product.

The Master: Riebeck ends where Ellis begins?

The Witness: Yes. Even then they are not the same kind of kerosene by any means, but rather similar. The only similarity I can see between the two is that Riebeck's product resembles the material with which the Ellis starts his process.

Q. I call your attention to the following statements of plaintiff's expert regarding the iodine numbers and unsaturation of benzol and similar aromatic hydrocarbons.

He stated on pages 348 and 349, near the beginning of his testimony, that benzol and toluol were unsaturated hydrocarbons, and that three tests were commonly used to distinguish between satur-ed and unsaturated hydrocarbons, namely the sulphuric acid absorption, iodine number and bromine number; unsaturated hydrocarbons responding to all three tests and saturated hydrocarbons not responding to any of them.

In discussing the Bassett and Boynton references on page 355 he apparently clearly implies that benzol and similar coal tar distillates are unsaturated and have high iodine numbers, so that they come within the claims of the Ellis patent.

Further, on page 356 he says that Williams' distillate was highly unsaturated because it contained large percentages of unsaturated hydrocarbons, such as benzol and toluol, but he states that he cannot calculate the exact iodine numbers because he does not know what other unsaturated compounds were present.

Later, on cross-examination, on page 656, he again states that benzol and similar aromatic ring hydrocarbons are unsaturated and that they respond to both the bromine and iodine tests.

He then proceeded to calculate the iodine number due to the presence of benzol in a saturated naphtha. Again, on

page 659, he said that the bromine number of pure benzol was about 36, but when asked to calculate the iodine number he, for the first time, expressed some doubt as to the readiness with which iodine reached with benzol.

Again, on page 665 he calculated that the bromine number of benzol would be 165. He was then asked to determine the bromine and iodine numbers of benzol and its mixtures with petroleum naphtha, and reported on page 791 that actually they were negligibly small for pure benzol and that the addition of one part of benzol to five parts of 70-degree naphtha decreased rather than increased the [fol. 1023] bromine and iodine numbers of the naphtha, directly contrary to his calculations on page 657.

Finally, in his re-direct, page 1135, he says that benzol is "well known to react with bromine and iodine with considerable difficulty, and indeed scarcely at all."

Now, having put all those statements together I want you to state which of them are true with reference to the iodine and bromine numbers of benzol?

A. The last statement that it is well known that iodine and bromine react hardly at all with benzol is the correct statement, and I believe the actual determinations which he gave near the end of his cross-examination are also substantially correct, I having made somewhat similar determinations, though of course not with exact checks.

The earlier statements as to the possession of a high bromine and iodine number by benzol are all incorrect.

Q. I also want to call your attention to some similar statements with regard to the sulphuric acid absorption test on benzol and similar compounds. I have already directed your attention to the fact that this is one of the three tests which plaintiff's expert stated might be used to distinguish between saturated and unsaturated compounds, and the statements in his direct testimony that benzol is an unsaturated hydrocarbon, thus implying that it responds to the absorption test; on cross-examination, page 656, he qualified this by saying that the aromatic hydrocarbons do not absorb in sulphuric acid as readily as do the olefins.

Then, on page 657, he said that benzol was not absorbed by 93 per cent sulphuric acid.

On the other hand, when asked to make the tests himself he reported, on page 791, that by a specified absorption test with 93 per cent acid he absorbed 51 per cent of pure benzol, and in a mixture containing 16-2/3 per cent of benzol in 70-degree naphtha, the increase in absorption as compared with that for the straight naphtha, indicated that about 90 per cent of the benzol was taken up by the acid.

On page 1052 he said that the test was kept cold by shaking the bottle in a suspension of ice and water.

Please state which of the statements regarding the absorption of benzol in 93 per cent acid is correct?

A. None of these statements as to the absorption of benzol are precisely correct in my experience.

If you follow carefully and accurately the directions for [fol. 1024] determining the sulphuric acid absorption of benzol in accordance with Technical Paper 181 of the Bureau of Mines, to which plaintiff's expert referred, and assuming that the several minutes shaking is actually five minutes shaking, (as it was later specified in the standard test when this was revised slightly in Technical Papers 298 and 323,) the absorption is about five per cent.

If you extend the time of shaking to fifteen minutes, as I believe plaintiff's expert did, the amount of absorption will be somewhat higher depending, to a considerable extent, on how much you allow it to heat up. However, plaintiff's expert, as I pointed out, said that he kept this in a mixture of ice and water, in other words, presumably around 32 degrees Fahrenheit. There is something very peculiar about this situation because plaintiff's expert apparently overlooks the fact that pure benzol freezes at 42° F., and so at 32 is a solid, and you do not get any kind of good contact if you try to carry out the absorption with a solid, which mainly freezes on the bottom and sides of the glass.

Trying to carry out the test under those conditions I found that no matter how long you shook it you could not get even five per cent absorption. So there seems to be something wrong with the determinations, either in the strength of the acid used or something of that kind. I have tried to find out what could explain it but I have not succeeded.

All of the acid absorption results which plaintiff's expert reported were high, including those on the straight run naphthas, although as I say I checked the bromine and iodine numbers fairly well. Therefore it rather seems to me that he must have been using a somewhat stronger acid, and if you go to somewhat stronger acid than that specified you will get considerably higher absorption figures.

Q. This absorption test in sulphuric acid is a standard test commonly made in oil refineries, is it not?

A. Yes, used every day in the large refineries.

Q. It is a matter of your regular, routine inspections, is it not?

A. On many types of products.

Q. And it is completely standardized by test methods prescribed by the technical departments of the Government, is it not?

A. Yes, it is a well standardized test which gives reproduceable results.

I might say that I have checked this absorption of pure [fol. 1025] benzol in two laboratories with two entirely different sets of reagents, both having samples of presumably pure benzol, and 93 per cent sulphuric acid as close as we could tell by testing.

Q. You get about five per cent absorption?

A. You get about five per cent if you carry out the test according to Technical Paper 181; but you really cannot carry out a significant absorption test on pure benzol cooled with ice and water, because the benzol freezes.

Q. In other words, this is a test for things that are liquid at usual temperatures?

A. No, the test itself does not specify that you carry it out by shaking it in ice and water. That was done to follow—I gave first the results of tests by the ordinary method and then the results obtained when shaking it in ice and water, as plaintiff's expert said he carried out his tests.

Q. And if you do it his way you won't get any absorption at all?

A. You get a little absorption but it is not comparable with a standard absorption test because you are not mixing anything particularly; you are just swishing the acid around over the top of the frozen benzol.

Q. In view of these facts, please state whether or not you agree with plaintiff's expert that benzol is properly considered to be an unsaturated hydrocarbon, and state your reasons?

A. I do not consider it to be an unsaturated hydrocarbon, and it is certainly not so considered commonly in the petroleum industry. It would be indeed a peculiar thing if it were considered unsaturated, because plaintiff's expert has correctly described the three tests which are used to distinguish between unsaturated and saturated compounds; and if we call benzol unsaturated, it would mean we call something unsaturated which did not respond to any of the three tests, except for a slight absorption in sulphuric acid.

In other words, by the three tests that are used in the petroleum industry, benzol would appear as a substantially saturated hydrocarbon. On the other hand, we do not call it saturated, either, if it does come into our ken in some particular special product. We refer to them as a distinctly different class of hydrocarbons, namely, the aromatic hydrocarbons.

A clear statement of the attitude of the petroleum industry in this matter is given on page 4 of this same Tech- [fol. 1026] nical Paper 181, which describes the determination of unsaturated hydrocarbons in gasoline.

Q. That is the Government publication?

A. That is a Government publication by the Bureau of Mines.

Mr. Royce: What date?

The Witness: The date is 1917.

The Master: Do you know who prepared the data for that paper?

The Witness: E. W. Dean and H. H. Hill.

The Master: Do you know them?

The Witness: I know Mr. Dean, and I think I have met Mr. Hill. Mr. Dean's office at the present time happens to be across the hall. He is now with the Standard Oil Company of New Jersey. He is a man who has been working in petroleum technology for a great many years—I guess he was with the Bureau of Mines for five or six years.

The Master: Both have a reputation for being careful investigators?

The Witness: Decidedly so. I think Dean's reputation is one of the highest of any man in the industry, for careful investigations. What I am interested in reporting here, though——

Q. I want to put just one other question about this publication. That is the publication, is it not, which describes and standardizes the routine tests normally used in the industry?

A. For unsaturated hydrocarbons.

The Master: The paper is accepted by chemists?

The Witness: The paper is accepted.

The Master: As sound practice?

The Witness: This was the paper which put the sulphuric acid absorption test on a well standardized basis, although it has been slightly modified since that time in a few details.

Q. Do you know who Hill is, Mr. Wilson?

A. I think that I have met Mr. Hill. I think he was Mr. Dean's assistant at the Bureau of Mines. I do not know where he is today.

I am quoting from a paragraph near the bottom of page 4 of this paper:

"The commonly accepted usage in the petroleum industry is to consider as 'unsaturated' only the olefin and the acetylene hydrocarbons. The term 'aromatic' is reserved for hydrocarbons of the benzene series, which although un- [fol. 1027] saturated as regards molecular structure differ radically from olefins in chemical activity."

I think this statement presents very clearly the view generally accepted in the petroleum industry, though from a theoretical organic chemical point of view the statement would be more accurate if, instead of saying that these aromatic hydrocarbons were unsaturated as regards molecular structure, it was stated that they were frequently pictured as having double bonds. That is, I do not think the picture which shows them as having double bonds is

really an accurate picture. That is the Ke Kule formula, which is I believe the oldest suggested for benzene, but it fails so completely to describe its behavior as compared with other compounds having double bonds, that most of us prefer some other type of formula which does not have double bonds.

So that I should not say that benzol was unsaturated as regards molecular structure, although it is sometimes pictured in that way.

Q. So that the industry in point of fact follows the terminology set forth in this publication of the Bureau of Mines and divides its products into saturated, unsaturated and aromatic?

A. Yes.

The Master: And puts benzol in the aromatics?

The Witness: Yes. In other words, when a man writing in this art speaks of unsaturated hydrocarbons, he never means to include benzol, so far as I know. Even in one of these exhibits which plaintiff's expert cited, page 1508 of the book of exhibits, Armstrong and Miller describe treating with acid of various strengths in order to complete the conversion of the unsaturated hydrocarbons, and after that they get out the pure benzol, which has not been converted, showing that they are not considering benzol to be an unsaturated hydrocarbons.

Also on page 1510 they say, "Much of the benzene may, however, be obtained without destroying the unsaturated hydrocarbons."

Again clearly distinguishing between benzene or benzol and unsaturated hydrocarbons.

Mr. Wiles: The technical paper No. 181 of the Bureau of Mines is offered as Defendants' Exhibit No. 418.

(The said paper so offered, was admitted and read in evidence, was marked "Defendants' Exhibit No. 418," and the same is herewith returned.)

[fol. 1028] Q. Plaintiff's expert also stated, page 350, page 655 and elsewhere, that one can readily calculate the iodine number of an oil from its bromine number, by multiplying by 1.6; and he also indicated, page 356, page 655

and elsewhere, that it is possible to calculate bromine and iodine numbers of compounds if you know the number of double bonds present, the number of carbon atoms per molecule, and certain physical properties such as specific gravity. Is that true?

A. No. That is just another example of the difference between theory and practice in this industry. Our chemistry is not nearly as clean cut and straightforward as we would like to have it. We get quite wide variations between bromine and iodine numbers. In fact, most of the literature on the application of bromine and iodine numbers to this industry is devoted to showing that you cannot calculate the bromine and iodine numbers from the number of double bonds, and that you cannot calculate bromine numbers from iodine numbers, or vice versa.

We do not need to go, however, beyond the data which plaintiff's expert himself furnished on page 791, to show how far from a ratio of 1.6 we may obtain. Thus the minimum ratio between the bromine number and the iodine number of a given product is that on benzol, which gives a ratio of 0.31 between the Huebl iodine number and the Allen 1881 bromine number, instead of the theoretical 1.6. It varies from this over a wide range, with quite scattering results up to a maximum ratio of 2.03, when we compare the Hanus iodine number with the Allen 1902 bromine number on shale gasoline.

Q. The ratio varies from one-third to over two?

A. From one-third to over two, and practically all the figures are quite a considerable distance from the factor of 1.6. The ratio between these two numbers is fairly good on animal and vegetable oils, where the iodine and bromine numbers were first used; but they simply do not follow the theory when applied to mineral oils. Iodine numbers of petroleum oils are usedful in empirical constants when applied in some specified manner and are a good check on the constancy of a given product but they do not give any direct information as to the exact number of double bonds present or what the bromine number of the same product might be.

Q. If, as a matter of fact, bromine and iodine both reacted with all the double bonds present, and if they re-

[fol. 1029] acted with nothing else, then you could make your calculations, could you not?

A. Yes, if those things which theoretically ought to be so, but which are not, were true, then you could calculate as plaintiff's expert indicated; but bromine apparently does not react in some cases with all the double bonds, and in many cases reacts with hydrogen, which is attached elsewhere and with which it theoretically should not react, and does not behave according to any definite theory.

Q. Plaintiff's expert also said on page 348 that the paraffin hydrocarbons had zero solubility in sulphuric acid. On page 657 he expresses doubt that a light naphtha with an iodine number of only 3 could give as much as five per cent absorption in 93 per cent sulphuric acid. On the other hand, he reported on page 791 that the 70-degree naphtha with a Hubl iodine number of 3.6 gave 12.5 per cent absorption in sulphuric acid, and when questioned about this on page 1055 he said that he had published about 1911 a paper which showed that sulphuric acid at room temperature would if agitated violently for a long time dissolve the saturated hydrocarbons completely. Now please indicate which of these statements regarding the solubility of saturated hydrocarbons is true in point of fact?

A. As I have already indicated, the sulphuric acid absorption test is also not an exact test for sharply distinguishing between unsaturated and saturated hydrocarbons. It is a fact, however, that most of the saturated hydrocarbons which we have in the petroleum industry, if subjected to the standard sulphuric acid absorption test, will show but very slight absorption, generally of the order of one or two or possibly three per cent. I am satisfied that the high figure which plaintiff's expert gives for the 70-degree naphtha is in error.

As to the paper which he published, showing that certain saturated hydrocarbons were completely soluble in sulphuric acid, I have looked through both the author and subject indices of the chemical journals without being able to locate this, although there undoubtedly is some such article which I have not been able to find.

But the facts are that the saturated hydrocarbons with which we ordinarily deal in the petroleum industry are

but very slightly soluble in 93 per cent sulphuric acid when tested in accordance with the standard procedure.

Q. Plaintiff's expert said on page 611, near the middle, [fol. 1030] that the products obtained in cracking shale oil "are the same essentially as those obtained in cracking well petroleum," and at the bottom of the same page this is repeated. On pages 702 and 703 he states on further cross-examination that cracked gasoline from shale oil is approximately water white in color, is substantially gum free, and has an odor about the same as ordinary cracked gasoline from petroleum. He also stated near the middle of this page that gasoline made from shale oil by the Burton process would be essentially like that produced by straight run distillation of well petroleum, though somewhat higher in unsaturated compounds. He also states in answer to the next question that he would expect that cracked shale oil gasoline made by the Burton process would be essentially the same as shale oil gasoline made at atmospheric pressure. Do you agree with these statements as to the characteristic properties of shale oil gasoline?

A. I believe that the last statement is essentially correct, that one would expect to obtain by the Burton process, starting with shale oil, a gasoline similar to that obtained from the shale oil directly, or in the process of making shale oil; because both are a certain type of cracking process (the formation of shale oil being a cracking operation) and both have very similar starting materials. I would not expect the products to be identical by any means, but I would expect them to be quite similar.

Q. In other words, you can crack shale oil under pressure and not under pressure to get about the same result?

A. To get about the same result. Of course, you would get quite different yields, but I believe your product would be quite similar.

When it comes to the statements regarding the good odor, good color and freedom from gum, and good stability of shale gasoline, I cannot agree. While I have not done a great deal of work in this field, I have examined a good many samples of shale oil gasoline and have done some work on cracking shale oil to make gasoline, and have tried

in some cases to refine these gasolines; and as a result of this experience I am very strongly of the opinion that some of the earlier statements of plaintiff's expert in his book and publication describe shale oil gasoline more accurately than some of those statements which you quoted in your question.

Thus, in his book page 145, in describing the difficulties [fol. 1031] of marketing shale oil products, I read from the second full paragraph:

"Shale products will have an odor considerably different from petroleum derivatives, and this odor may be more disagreeable than that of petroleum. Shale products may be more highly colored than similar products from petroleum; certainly they will be if excessively high refining losses are to be avoided."

Further, in an article in Canadian Chemistry and Metallurgy for January, 1923, by plaintiff's expert, we find quite a different picture of shale gasoline from that in his testimony here, he saying in the last full paragraph on the page,

"Shale motor spirit of Scotland often runs sixty per cent unsaturated, and accordingly it is strongly odored, somewhat colored, and cannot be stored for any considerable time without loss of quality."

Similarly, in Chemical Age, for January, 1922, page 3, referring particularly to this question of the character of the gasoline made by the Burton process operating on shale oil, I read from the paragraph just under the heading:

"Cracking crude shale oil," the following sentences:

"If the Burton process is used the gasoline so made will not be one of high quality. It will be strongly odored, will color on standing and will be inclined to carbonize in the cylinder."

Q. That is by plaintiff's expert, is it?

A. That is also by plaintiff's expert, yes.

Such statements are, I believe, essentially correct and emphasize the actual very great difference between ordinary shale gasoline and ordinary cracked or straight run gasoline from petroleum as marketed in this country.

Also in Industrial and Engineering Chemistry, for Sept., 1922, in an article by plaintiff's expert, page 848, I read from the first full paragraph in the righthand column:

"Accordingly, it is not surprising that when shale oil is further cracked to give a motor spirit, the product carries sixty to seventy per cent of unsaturated hydrocarbons."

This, of course, very clearly distinguishes it from the product with which we are ordinarily dealing, the cracked and straight run gasoline, to which plaintiff's expert compared shale oil in his testimony which characteristically contained not over twenty per cent of unsaturated hydrocarbons, and generally, in the blends which are marketed, [fol. 1032] partly cracked and partly straight run, are only eight or ten per cent unsaturated.

Q. Does that article show somewhere else how unsaturation was determined?

A. It is indicated on page 706 of the record that this means sulphuric acid absorption.

The big differences between the two products were further inadvertently emphasized by the plaintiff's expert when he offered in court a sample which he said was a typical shale gasoline, and which instead of having the characteristic 25 color which most of us demand of our gasoline when it leaves the refinery, or the 21 or 22 color which it generally has as marketed and received by the customer, had a color which on the ordinary Saybolt scale was actually below zero. You may recall those determinations being made in the court on the socalled typical sample of shale gasoline, which I believe was fairly typical of the ordinary shale gasoline.

Incidentally, this color was very poor, even though as plaintiff's expert later said, the end point of the gasoline was well below that of ordinary motor gasoline.

Q. And is it the heavy ends that give the high color, as a rule?

A. Yes, that means that the ends which would be most likely to be high colored, have been left out of the distillation.

Q. On page 1150, plaintiff's expert said that in his opinion a mixture of alcohol and ether would have been a suitable motor fuel for use in cars in 1912—is that true?

A. No, that is not true, for the reason that I pointed out, that a great many of the earlier cars had shellac covered floats, which were quickly attacked and put out of commission by any such mixtures. I am speaking of the floats in carburetors. They could only handle such fuel by replacing the ordinary float with some other type of float, or changing the coating on the cork.

The Master: But with a properly constructed carburetor to handle such fluid, could such a mixture be used?

The Witness: I think it probably could, if anybody wanted to pay the price. I have never tried such a mixture, so I cannot say from experience. It would depend on the relative proportions. Unless you had a lot of ether it would be hard to start, and if you did have a lot of ether it would tend to knock very badly.

Q. On page 1149, plaintiff's expert states that the end [fol. 1033] point specification on gasoline, which prevents the inclusion of material boiling above 437, is necessary to prevent difficulties in starting. Is that true?

A. No. The end point specification, as I have already pointed out, has nothing to do with the ease of starting, the end point specification being designed to prevent the inclusion of too much heavy ends, which would particularly condense on the cylinder walls and dilute the crankcase oil. The end point has no bearing, whatever on the difficulty or ease of starting.

Q. Plaintiff's expert said, on page 594 that the addition of propane to gasoline, would make a better gasoline—is that true?

A. No. Propane does not liquefy at ordinary pressures, until, I believe, 40 below zero Fahrenheit. If more than a trace of that is left in gasoline it makes what is ordinarily known as a wild gasoline, which tends to give off gas, and will interfere with the smooth operation of the carburetor, and not be a satisfactory gasoline. In fact, it is mainly propane which the casinghead gasoline manufacturers have found it necessary to largely eliminate from their product, in order to make it suitable as a blending agent to go into gasoline.

Q. On page 714 and 715, plaintiff's expert indicates that butane, propylene and butylene, as well as propane, are desirable constituents of gasoline. Is that true?

A. As to the propylene, that is practically as bad as propane. As to butane and butylene, small amounts of those can be included in gasoline without getting into difficulty.

But any considerable percentage of these compounds, will make trouble in summer operation, due to excessive gassing in the carburetor system, and, also, will increase the evaporation loss, and the hazard of storing gasoline.

Q. Plaintiff's expert stated that in reclaiming spent sulphuric acid in the petroleum industry, the cost of producing recovered acid of 75 per cent strength, is practically the same as making 93 per cent, or 66 degree recovered acid, because the acid must first be concentrated to the higher strength and then diluted back. Is that true?

A. No, it is not. In fact, it is very difficult to make a 93 recovered acid, but a comparatively simple matter to concentrate it up to about 75 per cent.

Mr. Raubel: What page is that on?

[fol. 1034] So far as I know, I never heard of the practice of first making the difficult 93 per cent acid, and then diluting back to 75 per cent acid. It certainly is not the common practice.

Q. Plaintiff's expert stated, on page 1013, that he would expect ordinary refinery safety valves, on ordinary atmospheric pressure stills, to be set at five to ten pounds. What is the fact?

A. The fact is that the ordinary practice, so far as our refineries are concerned, is to set the safety valves at about one pound pressure.

It is considered as a serious hazard, on those stills which are not built for pressure, and particularly those stills for coking operations, which get red hot bottoms, to carry more than about one pound pressure.

This was also verified by the witness Besselman, who testified that on ordinary atmospheric pressure stills safety valves were set at one pound pressure. Even on non-coking stills I don't believe we ever set them up above two pound pressure.

Q. On page 846, plaintiff's expert said that the cracked gasoline produced by the Burton process, was deficient in

light ends, and needed a light product, such as casinghead gasoline, to go with it, to meet market specifications. Is that true?

A. No, that is not true. The Burton process, as ordinarily carried out, makes a considerable excess of light ends, in comparison with what you can get from ordinary crude oils. As a matter of fact, it is only because we have had these Burton stills that we have been able to get along without buying large quantities of casinghead gasoline to increase the proportion of light ends, as other refiners have found necessary.

Q. The conditions are just the opposite of those he stated as the fact?

A. Yes, sir.

Q. Plaintiff's expert, on page 686 of his testimony, said that kerosene, when distilled at atmospheric pressure, was cracked to some extent, and upon this fact he based part of his conclusion as to the temperature at which cracking begins. What is the fact about the cracking of kerosene in atmospheric pressure distillation?

[fol. 1035] A. I am quite familiar with the phenomenon to which plaintiff's expert refers—of the darkening in color of kerosene, when it is distilled in the ordinary laboratory flask—particularly if it is distilled rather slowly, and, also, with the fact that you may get a little bit of heavy tarry material, which may be actually coked at the end of the distillation, if you keep the flame directed on the bottom of the flask.

It is not, however, an indication that cracking is taking place during this distillation.

The darkening in color is primarily due to the absorption of oxygen at the relatively high temperatures, and is particularly striking if you compare two flasks, as I have done, standing side by side, under reflux condensers into one of which you blow a stream of some gas which is free from oxygen, while you allow the other to be in contact with the air in the condenser. The one which has the oxygen-free gas going through it, to keep oxygen away from the kerosene, will boil for hours without appreciable change in color. Whereas, the other darkens quite rapidly even though the amount of oxygen which comes into contact with it, is not very large. These oxidized products are, of

course, rather non-volatile, and tend to remain in the very last end of the distillation, collecting in the bottom, and of course a small amount of contaminating products may be left in, right at the end of the distillation. If you continue the distillation until the bottom is substantially dry, and get the bottom red hot, you will coke this gum forming material, which has been produced during the distillation, and, also, a little of the heavy ends which condense in the upper part of the flask, and run down, may be coked by the momentary contact with the very hot bottom of the flask.

But properly speaking, cracking does not take place in the atmospheric distillation of ordinary kerosene.

Q. And that gives, therefore, no evidence whatever that cracking of petroleum products begins at the low temperatures that he stated that it did begin?

A. No.

Q. On page 643, plaintiff's expert said with regard to kerosene, that the flash point would be fairly close to the boiling point. Is that statement true?

A. No. As I have pointed out, there is, ordinarily a difference of 175 to 200 degrees between the flash point and the boiling point—that is, between the point where the liquid exerts a vapor pressure enough to give an explosive [fol. 1036] mixture, and a point much higher, where it exerts enough pressure to actually boil—displacing the atmosphere.

Q. On page 644, Dr. McKee calculated that the vapor pressure of hexane, at its flash point, would be one-sixteenth of atmosphere—is that true?

A. No, that is not correct. It requires between one and one and a half per cent of hexane to make an explosive mixture, and that means something in the way of one-seventieth, rather than one sixteenth of an atmosphere vapor pressure.

Q. That is, less than a quarter of what he calculated?

A. Yes, sir.

Q. In the same connection, on page 679, Dr. McKee insisted that compressing vapors before condensation, aided condensation, because it heated the vapors. Is that true?

A. No. The heating of the vapors is merely an undesirable concomitent. If we could compress without heating the vapors we would very much prefer to do it. The ob-

ject of the compression is to increase the boiling points, so that they will condense at higher temperatures.

Q. On page 980, plaintiff's expert said that hydrogen and methane are but very slightly soluble in oil, and to prove his point with regard to methane, he cites the fact that in natural gas wells undissolved methane sometimes comes out at pressures of a thousand pounds. Please state what, if anything this proves as to the solubility of methane and natural gas, and also, what the facts are.

A. Of course, this statement that there is undissolved methane at a thousand pounds pressure proves absolutely nothing as to the solubility of methane. It might be insoluble or it might be very, very highly soluble and yet have some left over which could not dissolve. Simply because there was not enough liquid to handle the available amount of gas. The existence of an excess of gas proves absolutely nothing as to what the solubility might be.

As a matter of fact, I have made some determinations myself but within the last few months the Bureau of Mines has issued a report, Serial No. 2732 in February, 1926, on the solubility and effects of natural gas and air in crude oils, and in this it takes some substantially pure methane and determines the solubility as being in most oils between forty and fifty per cent by volume at one atmosphere pressure.

[fol. 1037] That means at a thousand pounds pressure, such as plaintiff's expert referred to, a given volume of oil would dissolve something like thirty times its own volume of gas, measuring the volume of gas under ordinary pressures.

In other words, methane is really quite soluble in oil, as would be expected from its chemical similarity.

I might say that I have personally obtained figures of substantially this same magnitude.

Q. Plaintiff's expert said, on page 578, that an air-cooled bubble tower on a pressure still could not possibly operate well, or operate successfully to give a well fractionated pressure distillate without having a dephlegmator. What have you to say on that?

A. For the first year or two after I came with the company we had sixty bubble towers operating with only air cooling, and indeed, if they had been entirely uncovered we would have gotten so much more reflux than we wanted,

(instead of less as indicated by plaintiff's expert), that we would have had too light a product. Hence actually only the upper on third of these was unlagged, and yet we got enough radiation and condensation to give us a very satisfactory, well-fractionated product from this type of bubble towers with no dephlegmator.

Since then we have, as one of our steps of improveemnt, covered the towers completely and attached a dephlegmator, but it is absurd to say that you cannot get quite good fractionation and quite adequate reflux with an air-cooled bubble tower.

Plaintiff's expert apparently loses sight of the fact that we are working here at very much higher temperatures than ordinarily prevail in operation of bubble towers, and therefore the amount of heat radiated by the bare surface is very much greater.

Furthermore, it does not fluctuate as much with wind and weather as would the amount of heat transferred at lower temperatures, and actually you can care for practically any change in weather conditions simply by making some variation in the rate of firing.

Q. When you are running several hundred degrees above the surrounding atmosphere anyway, weather fluctuations make very little difference?

A. That is correct. The difference in heat transferred through the walls due to weather changes is very small. It [fol. 1038] is a thing that we can take up very readily in our variations in firing. It is desirable, although not necessary, to have some sort of a shelter to keep direct rain off the tops of the towers. That is really all that is necessary. Of course, you do get somewhat steadier and easier operation if you put on a dephlegmator, but the other gives very good fractionation and is satisfactory.

Q. Plaintiff's expert said on page 1098, that there would be marked variations in the temperature of different parts of a lead bath approximately a foot in diameter and two feet deep, and when pressed for justification of his statements, he said he had tested other baths showing variations of fifty or one hundred decrees in different parts.

On further questioning it appeared that the baths in which he found such variations of 50 to 100 degrees were air baths.

What have you to say on that subject?

A. Of course, the temperature variations in air baths bear absolutely no relation to the temperature variations in lead baths, because lead has more than one hundred times the heat conductivity of air. A lead bath is always recognized as giving a remarkably uniform temperature, but just to check up on the point I have actually explored different parts of a lead bath, with a thermo-couple, starting at the bottom, to see if there were appreciable variations, and I was not able to find variations of more than one or two degrees between right near the top of the bath and right near the bottom of the bath. It is remarkably uniform.

Q. And it is used because of its uniformity, is it not?

A. Yes, that is one of the main reasons for using it.

Q. On page 677 Dr. McKee says that Saybolt and Engler viscosimeters are both used for the commercial measurement of kerosene; is that true?

A. No, I have never known of either the Saybolt or the Engler viscosimeters being used for that purpose. They are designed for lubricating oils and while you can run kerosene through them, the readings are of no substantial significance because they are in a part of the range of the instrument where it is impossible to detect small variations.

The viscosity of kerosene, as I have mentioned, is actually determined by a special glass capillary instrument known as the thermo-viscometer.

Q. On page 625 to page 627 plaintiff's expert at several [fol. 1039] points indicated that cylinder oils were generally or at least frequently distilled from crude oil; is that true?

A. The term "cylinder oils" is, so far as I know, universally used in the industry to refer to the lubricants made from the heavy, undistilled residue left after you have distilled off with steam practically everything that will go off from the crude oil below a cracking temperature.

I have, within the last few months, seen what would almost be considered half-humorous references to a very high vacuum process, by which you could make distilled cylinder oils, but that is a scientific curiosity; and certainly, the term "cylinder oils" as used in any of the references always means a residual product which has never been distilled.

Q. Now, turning from these miscellaneous details which I mentioned in the beginning, I want to call your attention to certain substantive features of distillation, mainly two in number. I will ask you whether, in addition to the features which you have pointed out as present in the patents of the primary defendants offered by the plaintiff, you find the following features in any of the processes of the defendants, and if so in which: In a coil and reaction chamber type of process, the feature frequently referred to as "clean circulation", in which both the tar and the vapors are separately withdrawn from the reaction chamber and the condensate from the dephlegmator or runback is continuously returned to the coil for re-treatment during the operation of the process?

A. Yes, this feature is present in the Holmes-Manley process, where, as I indicated, we have a coil and reaction chamber type of process in which the tar and vapors are separately removed from a series of chambers, or what amounts to one sectional chamber, and the condensate from the de-hlegmator is put back through the coil to pick up some heat before it gets to the still.

Q. In that type of process is the chamber made in sections arranged in series?

A. Yes.

Q. And the oil passes through in series?

A. Yes.

Q. And the oil is forcibly introduced into the coil by the surge pump which you described?

A. Yes.

Q. And is that feature also in the continuous process of the defendants, Standard Oil Company of Indiana?

A. Yes, it is.

Q. In addition also to the features which you have pointed out as present in the patents of the primary defendants offered by the plaintiff, do you find in any of the processes of the defendants, and if so in which, the following feature: That which includes heating the oil in the coil to a full cracking temperature but postponing the bulk of the cracking to the reaction chamber where little or no heat is applied. This is sometimes epitomized as heating the oil without cracking and cracking without additional heat.

A. This type of operation is customarily carried out in

the operation of the Cross process, where although they heat to a very high temperature they keep the rate of circulation so high that very little cracking takes place in the coil, it practically all being postponed to the unheated drum.

The same effort is made, as I pointed out, in the Holmes-Manley type of process, though in the continuous stills at Whiting the arrangement of the piping has been changed somewhat, so as to give rather substantial cracking in the coil in addition to the cracking which takes place in the drums.

Q. In that respect, then, the continuous process at Whiting differs from the ordinary Holmes-Manley process?

A. Yes.

[fol. 1040] Q. What do the primary defendants do of value to the licensees in the way of giving them, if they do give them, instruction as to engineering details, safety measures, minor details of operation, construction drawings, and things of that kind, relative to the processes?

A. All four of the licensors furnish to the licensees a great deal of material, which has a great deal of value, in addition to the licenses themselves. This includes, in the first place, the furnishing of complete detail drawings of the latest forms of equipment, which of course would cost thousands of dollars to duplicate, even starting with the patents, which furnish the essential features of the process.

The Master: Do you mean working drawings?

The Witness: I mean working drawings. Take the Holmes-Manley still, for example. I should assume that even knowing all the essential features of the process, it would cost at least fifty thousand dollars and maybe a good deal more to prepare the detailed working drawings of a commercial installation.

Furthermore, there are a great many alternative methods of carrying out certain things. The questions of what kind of pumps to use, what kind of valves to use, what kind of return bends should be employed, are all such that actual experience is the only way you can be sure which is best, and that experience is transmitted in these drawings. Also it is the common practice to furnish a man to supervise the construction and the inspection of the equipment,

a man who is familiar with the requirements of such construction, which is rather different from ordinary refinery construction, and who knows the importance of certain inspections on the materials as they are delivered and of tests to be made on the plant after it is assembled and before it is operated.

Also it is common—I do not know whether it is universal or not—to furnish starting gangs to start up a series of stills for the first time and to instruct the licensee's own men in the details of operation. There are, of course, a tremendous number of valves and lines and pumps which have to be coordinated to get a satisfactory operation of the process, and it is vary hard for green men to do that without making a mistake which might not only be expensive but might be actually fatal, because we are dealing with a very real hazard in the form of oil superheated to very high temperatures and pressures.

In addition to this they furnish the preferred operating schedules for diffeernt types of stocks. Of course, there are a great many schedules which would work, but by long experience we have found that the maximum return from the equipment can be had by adhering fairly closely to certain schedules. Cleaning methods and cleaning tools for the tubes and other parts are also furnished, and save a good deal of experimenting with different types of devices.

The Master: Some of these cleaning tools are patented?

The Witness: No so far as I know by any of the primary defendants. They may be patented and very probably are by some of the companies which make these tools.

One of the most important things is the safety measures which have grown up around these processes and have been found necessary as a safeguard. The ordinary inventor who develops a process does not think very much about that; it takes another type of mind generally to come in and throw around it the safeguards which the ordinary man would never think of, such devices as long distance control on certain important valves, so that they can be operated from behind some shelter; spare pumps, the building of fire walls around the separate units to prevent oil overflowing from one place to another; the protection of the supporting steel structures with concrete so that in case of fire they do not weaken and fall—and a great many devices of

that kind whose omission might be extremely expensive and even cost lives. That is part of the service furnish.

And then, of course, there is a general engineering service—advise on different types of equipment or improvements to be made, the many little kinks in construction which, being out of the ordinary refinery line, are quite new [fol. 1041] to the men; how to make the joints between the pipes to stand these very high pressures and temperatures; what kinds of packing will really stand the gaff in service. And in addition to that an up-to-the-minute service on improvements as they take place. Many of the improvements as they are made have patents applied for, but may take a long time to get through the Patent Office, whereas the licensees are kept up to the minute on any improvements which have been made by the development staff of the licensor company.

The Master: Laboratory service furnished?

The Witness: Just what do you mean?

The Master: I mean does the licensee give the right to ask for laboratory service—chemists?

The Witness: Not ordinarily. They do have the privilege, of course, of coming in and inspecting our operations, and have full access to the records of our results.

Q. Many improvements that are worked out in your laboratory they get right away?

A. Yes. At least as soon as they are proven to be successful on a large scale. We do not bother licensees with them at all until it is proven, as to whether they are good or bad.

The Witness: We have here a model of two units of a typical tube and tank installation, as designed and developed by the Standard Oil Company of New Jersey. This model is also on a scale 1/24 full size, except that the pipes are not exactly to scale, though the essential parts of the apparatus are.

I may say, also, that I am not so familiar with every detail on this model as with the Holmes-Manley, because I have not lived with them as I have with both the Burton and the Holmes-Manley.

The Master: Is this something of the same thing that Mr. Howard showed me over at Elizabeth?

The Witness: Yes. Starting back at the pump house beneath the condensers we have the cold feed pumps, which take suction on a small tank of stock and pump the oil at a pressure of around 400 pounds through the green line indicated.

We have here two substantially identical units with certain cross connections that can be used for special purposes, but I am only describing the operation of one unit.

That feed line passes over to an arrangement in the top of the bubble tower which serves to condense the heavier cracked vapors which discharge into that tower, as we shall later see; and also to pick up the corresponding amount of heat in the food. There is a bypass valve so that the amount of the feed which goes through the top of the bubble tower can be regulated, thus regulating the composition of these vapors which leave the top of the bubble tower.

[fol. 1042] The feed having picked up this much heat then passes past the soaking drums into the pre-heater through the line to which I am pointing (indicating).

The Master: These are the soaking drums?

The Witness: These are the soaking drums.

The passage of the oil through the pre-heater can be seen most clearly on the other unit.

This shows the piping arrangement with the feed oil coming in from the lower part, just before the flue gases leave, rising through the bank of tubes, each of which is connected by a return connection; then passing down just above the fire, and then passing parallel to the flow of gases for some distance, leaving from the top rear end of the furnace.

You will recall, as I pointed out, that the tube and tank process does more cracking in the tubes than any other commercial process with which I am familiar. They deliberately do a considerable amount of cracking in the tubes, and therefore have to clean their tubes somewhat more frequently than some of the other processes.

I should say that the total feed capacity of one of these units is in the neighborhood of eighty to ninety thousand gallons per day. That appears to be a little larger than the Holmes-Manley capacity, but you will recall that in the Holmes-Manley there was considerable re-cycling through the tubes within the system, whereas here, all the cycle stock comes out and passes through the feed pump a second time

and is therefore counted twice; so that actualy the fresh feed capacity of these is lower than that of the units you saw the other day. Of course, the cost is also considerably lower than the cost of the Holmes-Manley units.

The Master: What do you mean by "cost"? You mean cost of construction?

The Witness: Cost of construction.

The oil vapors leaving the pre-heater then pass over through the line I am indicating and into a branch line so they can go in parallel to either one of two soaking drums. Each unit has two soaking drums. Sometimes they are operated simultaneously in parallel, and sometimes they are operated alternately, allowing one soaking drum to be filled up with coke and then switching to the other while the first one is cleaned, and so on. That depends on the type of stock being handled and the temperature and so forth at which it is being operated.

The unit is quite a flexible unit, being able to handle quite [fol. 1043] different stocks with the production of different types of results.

There is also an emergency line which permits, if desired, the feed to go directly past the soaking drums instead of going through them.

I should also point out that the line into the soaking drum is equipped with an automatic check valve so that while oil can go into the soaking drum through this line, if there should be a split tube or anything else happen to the pre-heater part of the system, the contents of the still would not be discharged out through that break, thus greatly reducing the amount of oil which would be involved in a fire due to a split tube, or similar accident in the heated part of the system.

The soaking drums are six feet in diameter by forty feet high, and as a rule, are heavily lagged to maintain a high temperature as long as possible.

The mixtures of liquids and vapors are withdrawn from a pipe which takes off a short distance below the top of the stills but runs down inside the stills and actually comes out from the shell at the bottom. Immediately beyond that are two valves one or the other of which is used to control the pressure in the soaking drums, generally at 350 pounds.

The liquid discharged from one or both of these soaking drums then shoots into the base of the large bubble tower

shown behind the soaking drums, the bottom of this chamber serving essentially as a separating drum for the separation of the unvaporized tar and the cracked vapors. That tar settles to the bottom and is withdrawn through the tar cooler shown on one side of the condenser box. The vapors which rise through this tower pass first through a series of bubble plates in the middle section of the tower and then come in contact with the cold feed which serves as a partial condenser for those vapors, and keeps back the intermediate material which is lighter than the tar but heavier than the desired product. This material thus condensed comes down through the bubble plates where it is stripped free of the desirable light ends and is caught in a collecting pan, such as I sketched on the drawing, and is drawn off through this line to the cycle stock coolers, also located in the condenser box, and from there it goes back to the feed tank and is put through again.

I might say that these bubble towers are ordinarily operated at a pressure of about fifty pounds. The vapors under this pressure pass out through the yellow line and into another section of the condenser box, and then down into the receiving house below, where you can see the small separating drums, which are also yellow in color.

I note the separating drums do not show the gas line, which is however, a small line which is actually used on those drums to separate the gases from the liquid under a pressure of about fifty pounds, and thus save as much as possible of the desirable light ends in the gasoline.

Most of the other lines which you see are lines for steaming out or for the emptying out of the system at the end of a run.

[fol. 1044] The Witness: There is one point of interest, with regard to the tube and tank unit, which I didn't mention, and that is in conenction with the coke cleaning. They may have a large accumulation of coke in their soaking drums, and various methods have been tried for the removal of that coke. Of course, the surfaces not being fired, and not being equipped with scrapers, it is quite a different problem from that of the Holmes-Manley still.

One of the schemes that has been used is to coil a chain around the inside walls of the drum, on light supports, and then, at the end of the run, get hold of the chain and pull it out, breaking up the coke, and bringing it out. But that

didn't work entirely satisfactorily because the chain frequently broke, under certain conditions, and the preferred method now employed, is to have a boring machine, which is sunk in a hole in the ground, in the preferred type about sixty feet deep, and which is equipped with a large drill, which passes up and drills out a hole the size of the manhole. Then certain expanding arms, which collapse to go through the manhole, and then expand out, are put through to make a second cut to within a short distance of the rivet heads. The remaining coke is then loosened by brass discs on chains which are frequently used, rotating at high speed, the discs striking and chipping off the choke. Of course there is no need for such extreme care in cleaning of the surface, in this unit, as in the Holmes-Manley process, because the surfaces are not fired, at all.

The Master: The returns on the tubes, are made, also, so that they can be taken off and the tubes scraped?

The Witness: Yes, sir, that is practically the universal practice, although in the case of the Holmes-Hanley still not all the tubes are fixed for cleaning.

Q. Now, Mr. Wilson, when you took up the Barbet patent in detail, you were going to have a drawing made with your legends in ink on them. Have you done that?

A. I made the sketch and from that the lettering has been put onto the drawing.

Q. And these are the same legends which you had on when you referred to it, plus some more?

A. Yes, sir.

Q. And they are all self explanatory?

A. Yes, and they are based on the disclosures in the patent.

Mr. Wiles: I offer in evidence this photostatic copy as Defendants' "Exhibit No. 419."

The said photostatic copy of sketch, so offered, was admitted in evidence, was marked "Exhibit No. 419," and the same is herewith returned.

Q. What is the fact as to whether the gasoline made by the Cross licensees corresponds with the gasoline of the Ellis Product patent.

A. The gasoline thus made I have examined, and after refining it is a characteristic gum free, fairly high end point, good color product, still containing olefins, characteristic of Ellis' disclosure in his product patent.

Q. And how about the treating generally employed if you know?

A. The treating generally employed, by Cross licensees, I have been informed and I know specifically of a few instances, is comparatively dilute acid, such as the industry is now almost universally using on its cracked products.

Mr. Wiles: Now, aside from some possible point that may turn up during the Government's cross examination of its proposed witness, they may cross examine the witness. However, there is one point I want to go over before I formally close.

I now offer in evidence this diagram—a small black and white diagram—as "Defendants' Exhibit No. 420," with leave to refer to the larger diagram, at any time, as a more graphic illustration.

The said diagram, so offered, was admitted in evidence, was marked "Exhibit No. 420," and the same is herewith returned.

[fol. 1045] Mr. Wilson: On page 5086 of the Typewritten Record, that of Tuesday morning, I made a statement that to get equal strength in an eight foot still, to correspond to a six-inch pipe with half inch walls, would mean still walls a foot thick. That was an arithmetical error. I should have said which would mean still walls eight inches thick—which would not change my conclusions, particularly.

On Monday afternoon, October 11, on page 4957 of the Typewritten Record, I stated that all bulk pressure distillation processes with which I am familiar, operate between 720 and 820 degrees Fahrenheit. I did not make it clear, according to the transcript, at least, that I had reference to the temperature of the characteristically large body of oil in these processes. Of course, in a process of the Holmes-Manley type, the stream which comes into the still may be quite a little higher than that, as may be desired. But the statement is accurate, as I meant it to apply to the body of oil.

Also, in speaking of my results on sulphuric acid absorption of benzol being very low, and benzol not being considered an unsaturated hydrocarbon, and in referring to the Technical Paper No. 181 of the Bureau of Mines, which was offered in evidence as "Exhibit No. 418" I meant to refer also to the table on page 15, where the men who developed this sulphuric acid absorption test, definitely tried it out on a mixture of benzene, and uncracked gasoline, and showed that the mixture only gave one per cent absorption. In other words, they had a table of results of experiments, showing the slight effect of sulphuric acid on aromatic hydrocarbons.

Cross-examination.

Questions by Mr. Rauber:

Q. Mr. Wilson, the first degree you got was at Wooster College, in Ohio?

A. Yes, sir.

Q. What sort of a course did you take there—scientific, or arts?

A. I took what was known as the philosophical course, which is about half way between, but I specialized in chemistry and physics.

Q. Did you have any experience in oil work before you received your degree at Wooster?

A. No practical experience, no.

Q. Anything more than the average person?

A. No.

Q. Then you went to the Massachusetts Institute of Technology, at Cambridge?

A. Yes.

Q. What year did you go there?

A. The fall of 1914.

Q. And you got credit for your work in Wooster?

A. For some of the work, yes.

Q. About how many years' credit did you get in that way?

A. I don't know—I never paid any particular attention to that. I wanted certain courses at Tech, and I took them.

Q. You got a Bachelor's Degree at Technology, in 1916?

A. Yes, sir.

Q. You did some research work in 1915, did you?

A. Yes, in the summer and fall of 1915.

Q. Was any of that concerned with oil?

A. No.

Q. Did you write a thesis in 1916?

[fol. 1046] A. Yes.

Q. Was that thesis based on any of this research work?

A. It was based to a considerable extent, on this research work in the summer of 1915, although I was paid for that work in the Research Laboratory of Physical Chemistry.

Q. I suppose this thesis was about the average undergraduate thesis, was it?

A. Modesty forbids my characterizing it. I will say that it was published in two parts, in the Journal of the American Chemical Society, which is not the fate of most undergraduate theses.

Q. You took Course X in Chemical Engineering?

A. Yes.

Q. About how many students graduated at the same time you did?

A. About forty-five or fifty, I should say, in chemical engineering.

Q. O- course there were other graduates in other departments?

A. There were many others in other departments, yes.

Q. After you graduated you did some further work at the Massachusetts Institute of Technology, did you not?

A. Yes.

Q. And that was done for and paid for by corporations, was it?

A. Well, if you are going chronologically, you skipped a period in which I worked for the General Electric Company.

Q. Oh, yes, but you afterwards returned?

A. Yes, I afterwards returned to Technology.

Q. And that work was done for corporations, was it?

A. No, I started to work on fundamental problems, and then I did, at Dr. Walker's request, handle some chemical engineering problems for outside concerns.

Q. They were firms that had subsidized the Institute, and had students, or others, working on their problems?

A. I don't know just what you mean by subsidized the Institute—they paid certain amounts to the laboratory, to have certain problems worked on. We just drew our regular salary from the laboratory.

Q. Did you ever receive post-graduate degrees for any of that work?

A. No.

Q. Did you receive any post-graduate degrees, at all?

[fol. 1047] A. No, not in the sense of a degree, higher than Bachelor.

Q. That is what I mean. That, of course, is the lowest degree granted for scientific work, isn't it?

A. I presume so.

Q. When did you first enter into work for oil corporations?

A. Not work with oil, but work for oil corporations, you are asking about?

Q. Well, concerning oil.

A. In the chemical warfare service I did quite a bit of work on incendiaries.

Q. That is, burning oil to get a flame, isn't it?

A. Yes. We had to study the properties of oils for that purpose.

Q. When did you do any other work since that in the chemical warfare service, relating to oil and oil refining?

A. The first work that I did directly relating to oil refining was immediately upon my return to Technology, when I undertook work for the Vacuum Oil Company.

Q. When was that?

A. That was in March, or April, 1919.

Q. That was your first contact with actual refinery operations, was it?

A. Except such casual contact as you get by going through an oil refinery, which, of course, does not amount to much.

Q. How long did you work for the Vacuum Oil Company, on that work?

A. Until sometime in the fall of 1921, or the early part of 1922.

Q. You worked about two years?

A. More than two years.

Q. For the Vacuum Oil Company?

A. That is, when you speak of me working for them, I was assistant director and later director of the laboratory, and generally three of my men were immediately on that problem, and I, personally, kept in very close touch with it,

and frequently visited the refinery in connection with that work.

Q. What type of work were you doing—what was the nature of it?

A. Quite a number of different problems came up. The actual problems on which we did most of our research, were in connection with the refining of lubricating oils. They discussed with me problems of distillation—of vacuum distillation—and we also did quite a bit of work on making [fol. 1048] better barrels, drying the staves better, and sizing the barrels better.

Q. Did you do any other work except that for the Vacuum Oil Company, between 1919 and 1921?

A. Any other work on oil, do you mean?

Q. No, for any other companies or corporations.

A. Our laboratory did work for a large number of different corporations, which I believe I mentioned.

Q. How much of your time did you devote to oil research in that period?

A. Oh, I should say 20 to 25 per cent on the Vacuum Oil Company, and then we did quite a bit of work—General Motors, and for the Standard of New Jersey, and I handled one or two consulting problems on oil.

I should say, all together, that probably 40 per cent of my time was on oil problems, because I was, by that time, quite interested in the oil industry, and concentrated particularly on that.

Q. But none of it had anything to do with cracking, did it?

A. Yes, I made a report for a group of men in Boston, who wanted to consider cracking Mexican crude, and wanted to know what the possibilities were, and in the course of that I visited the Bureau of Mines and discussed with them what cracking processes were available, and what kind of stocks could be cracked, and I also talked to the Pan American people, regarding the cracking of such stocks.

Q. In what particular process?

A. I found, at that time, practically the only thing available was the Burton process—the only thing that was in commercial use.

Q. That was when?

A. 1919—as far as I could learn, at least, from the Bureau of Mines—and that would not handle the type of stocks I was interested in, directly.

Q. When did you leave the Vacuum Oil Company,—in 1921?

A. I didn't leave them.

Q. You worked for them—what did you do then?

A. We started in working for the Standard of New Jersey, more or less in place of the Vacuum Oil Company; and also for the Humble Oil Company. That is, most of our work for the Standard was really done more directly for the Humble Oil Company. That was some time in 1921, as I recall it.

[fol. 1049] Q. Since that time you have been working for the Standard Oil Company of New Jersey or Indiana, which?

A. The work that I was doing at that time was for the Standard Oil Company of New Jersey and the Humble Oil Company.

Q. How long did you work for them?

A. The work was still going on at the time when I left.

Q. When did you leave?

A. In the fall of 1922.

Q. Then for whom did you work?

A. Of course, again you are skipping certain things. I do not know if you wish to.

Q. What am I skipping?

A. The work we did for the General Motors on oil problems.

Q. That was carburetors?

A. That was in connection with the volatility of different fuels and on lubrication problems.

Q. Then you went to work for the Standard of Indiana?

A. Excuse me, I do not mean to interrupt, but are you trying to get a complete outline of my oil experience or do you——

Q. Of only the oil experience.

A. I also did some work for the Yale & Towne Manufacturing Company on an oil problem involving trying to commercialize a kerosene carburetor on which they were working, which involved problems of accomplishing vaporization and yet preventing cracking and getting proper mixtures, and preventing the dilution of the oil in the crank case of these cars.

Q. When did you first start working for the Standard of Indiana?

A. In the fall of 1922.

Q. What has been the nature of your employment since that time?

A. I have been assistant director of research in charge of chemical engineering and automotive engineering problems.

Q. Who was the director?

A. We have no director. The directing is done by a board of three who have charge of the different branches. Each of us is practically a director of our own particular branch of the work.

Q. In your position as assistant director you have access [fol. 1050] to all of the reports of the Standard Oil Company of Indiana?

A. So far as they can be found. Theoretically, the filing system gives access; sometimes we cannot find just what we want.

Q. Do you think that you are reasonably familiar with all of the reports?

A. No, I simply looked into the reports on past work in which I was particularly interested.

Q. Did you see a report on the Adams process?

A. I have, generally, when I wanted some of the data from that report, called in the man who actually did the work to get his tabulated data, because he could dig it out quicker than I could. I have never read the whole report, but I have inquired about certain features of the process repeatedly.

Q. The Adams process was tried out by the Standard of Indiana.

A. Yes.

Q. When?

A. Some time before I came with them, I believe before the first Texas-Indiana agreement. I cannot give the exact date. I should guess it was around 1919 or '20, but I really don't know.

Q. Can you tell me the names of the other men on that board of three directors?

A. Well, the members on the board, if you want to go into the intimate details——

Q. Not too intimate, just to know the other two names?

A. Well, to simplify matters I have not given all the details, but if you want exactly the situation it is as follows:

When I went there Dr. Wendt and Dr. Rogers and I were the three men who had charge of the different branches of the work. Then Dr. Wendt left and Dr. Sullivan took his place, and then Dr. Sullivan, Dr. Rogers and I handled it.

On account of the press of work in this case, which has necessitated my being away quite a bit of the time, three of the men who were working under me have been given the position of assistant directors and I have been made a special assistant to the management though I am supposed to direct their work just as much as I can.

Those three men who are under men are Barnard and Whitman and Bahlke and they now rank as assistant directors.

[fol. 1051] Q. Have you been working for any other company but the Standard Oil Company of Indiana since you went with them in 1922?

A. No.

Q. All of your time has been taken up with their work?

A. Yes.

Q. You stated that you testified in the Dubbs case for the Standard Oil Company of Indiana?

A. Yes.

Q. About how long did you testify?

A. I think about three days.

Q. Were you the only expert that testified for the Standard of Indiana?

A. No, I was not.

Q. Who else testified?

A. Dr. Raymond Bacon, Dr. Milliken of the California Institute of Technology, and Dr. Paulus of our company, and Colonel Allen.

Mr. Wiles: Colonel Henry Allen of Chicago.

The Witness: I think those were the only experts that we had.

Q. Dr. Bacon was the principal expert, was he, on the technical features of oil cracking?

A. Well, the three of us covered rather similar ground. That is, Paulus, Bacon and I covered rather similar ground.

Q. But they testified at much greater length, did they not?

A. I do not believe they did, on direct. On cross-examination it varied quite a little in length. But they may have;

I won't say. It is quite possible Dr. Bacon's direct was longer than mine.

Q. At page 5723 of the typewritten transcript of record you mentioned seven features that were first disclosed by Mr. Adams.

A. Yes.

Q. The first of those was the indirect heating of a bulk supply of oil.

A. Yes, in that type of process.

Q. That is a bulk pressure distillation process?

A. For making gasoline.

Q. In which of defendants' processes is this feature used? Before I ask that question, will you tell me just what you mean by indirect heating?

A. I mean the application of heat to a flowing stream in a [fol. 1052] constricted passage, and thereby keeping hot a large body of oil which is either not heated directly or only lightly heated.

Q. All right, now if you will answer the other question.

A. What was that page, please?

Q. Page 5723, at the top of that page; right at the beginning there.

A. In the narrower sense it is used in a bulk pressure distillation process in a manner rather closely analogous to that of Adams in the Holmes-Manley process; in a broader sense the same invention is, of course, found in the tube and tank process and in the Cross process and in the Clark tube still. Of course, the tube and tank type of process and the Cross type of process are not bulk pressure distillation processes.

Q. In each case the indirect heating consists in having a heating coil through which this liquid flows from and to a bulk supply of oil?

A. No, that is true only of the Clark tube still.

Q. In the others it consists merely in passing the oil through a heating coil and into the bulk?

A. Yes.

Q. The distinction between the narrower sense and the broader sense is that in the narrower sense there is no distillation from the bulk supply?

A. In the narrower sense there is distillation from the bulk supply. In the broader sense that feature is not included as a limitation.

Q. I call your attention to Exhibit 405. Will you point out the tubes and the bulk supply in the drawing.

A. The tubes are so marked as tubes and the bulk supply is in the still above the tubes. The same is true of the high pressure still, essentially the same arrangement, except that there part of the bulk supply is in unfired drums at the rear and front of the bank of tubes, as well as above them.

Q. Is there any digestion takes place in that bulk?

Mr. Wiles: Which exhibit is that?

Mr. Rauber: Exhibit 410.

The Witness: That is Exhibit 410, yes.

A. Just what do you mean by digestion?

Q. In the sense in which Ellis uses it.

A. I am not familiar with exactly the sense to which you refer. Do you mean further cracking?

Q. I think that is what he meant, isn't it?

[fol. 1053] A. Yes, I think that is what actually took place, certainly, in Ellis' disclosure.

Q. And that is what he means by digestion.

A. I explained that at considerable length in my direct. Apparently in the early stages of prosecuting his patent he thought the reactions which took place in the drum were qualitatively different from those which took place in the tubes; and that was reasonable, because the qualitative results of a tube and tank type of process were different from a straight tube process, but actually that difference is due more largely to further cracking, although there is a certain stabilization which he apparently thought was the principal thing which took place.

Q. In the Holmes-Manley process shown in Exhibit 411 you have what you call the heating coil?

A. Yes.

Q. And the bulk consists of those vertical chambers?

A. The bulk supply of oil is in the sectional still, yes.

Q. And in the Cross your tubes are not marked, are they —in Exhibit 412?

A. No, it is plainly evident what the tubes are, and there is a large body of oil in the reaction chamber.

Q. And the tubes in the tube and tank process of Exhibit 413 are marked "cracking coil"?

A. Yes.

Q. And the digestor is marked as "Soaker, or digestor"?
A. Yes.

The Master: Could the polymerization which you said took place in the Holmes-Manley stills in any sense mean the same thing as digestion?

The Witness: No. I think Ellis was referring mainly to the character of the lighter product which came off and not to the heavier products, which characteristically form and stay in the still in a bulk pressure distillation process. The character of the lighter products themselves is different in a tube and tank type of process, as I pointed out, from what it is where a straight tube process is doing an equal amount of cracking.

Q. I would like you to point out where any of these features are shown in the Adams patents.

The first Adams patent is Exhibit 185, and its number is 1,320,354.

[fol. 1054] The Master: And the reference was to Fig. 2 of that patent?

Mr. Rauber: Any figure in which he can find it.

A. Take Figure 1, for example, of this patent. We have the restricted heating zone right after the oil comes in, which is in this case the electrical heater, and the bulk supply of oil, above it, the liquid level being shown up in the Dome E.

Q. That heater that you speak of is marked A?

A. I do not see anything marked on it in Figure 1. It is in the chamber marked A, yes.

Q. That chamber corresponds to the pipe coil in the Cross and Holmes-Manley and Clark?

A. So far as this feature is concerned it does correspond to the coil, yes.

Q. Where is the bulk shown in this Adams patent?

A. The upper part of A, above the heating zone, is part of the bulk supply, and the liquid which is shown about half filling the chamber E, is the remainder thereof.

Q. That is the enlarged portion is the bulk, the part that is marked E?

A. No, both the upper part of the chamber A and what liquid oil is shown in the chamber E, constitute the bulk supply.

Q. The part that corresponds to that long tube in the Holmes-Manley and the Cross and the Clark processes is just that portion around the electric heater?

A. Yes. That is the restricted heating zone of Adams. In the externally fired units, of course, that takes somewhat different forms as shown by the different drawings of this patent.

Q. That is exhibit what?

A. Exhibit 185. Do you mean the other one?

Q. No, the next one. Exhibit 186.

A. That is patent No. 1,327,263, the main process patent.

Q. That is Exhibit 188.

A. Yes, Exhibit 188. In this case the restricted heating zone is, of course, that within the furnace walls.

Q. That is the part between the wall 12 and the cylinder 68?

A. Yes.

Q. And the digestion chamber is the enlargement having the letter X on it?

A. Yes, the unheated bulk supply is the oil below the line [fol. 1055] X, I suppose extending clear down to the end of the heating zone in the narrower passage.

Q. Well, the narrower passage between the cylinder 68 and the wall 12, that corresponds to the coil, doesn't it?

A. Not after you have left the heating zone, I should say, in a strict sense. I pointed out, for example, in the high pressure stills, that part of our bulk supply was really down at the immediate ends of the two banks of tubes as well as the main drums up above.

Q. In the Holmes-Manley your digestion, then, begins immediately upon leaving the heating coil in the furnace?

A. Well, again, by "digestion" do you mean further cracking takes place without considerable application of heat?

Q. Whatever it means. There is application of heat in the Holmes-Manley, isn't there?

A. I said without considerable application of heat; without heavy firing, as we frequently call it.

Q. I think that is what they mean, isn't it?

A. I do not think Adams speaks of digestion at all. I do not recall using the term "digestion" in connection with the Holmes-Manley process.

Q. That is the only place you find that feature disclosed, is it?

A. In Adams?

Q. Yes.

A. Oh, no, he has a good many disclosures, as I recall it. Those are the two principal patents, but I am not so familiar with some of the others. It may take a little longer to find those.

The Master: How about Patent No. 1,320,727?

The Witness: I was just going to look at that one.

Mr. Wiles: Is that in evidence?

The Witness: Yes. I will have to puzzle out that line of flow there. It rather looks to me as though he had a similar constructed heating zone and bulk supply, I mean functionally similar, although arranged in a different way. But I would have to read through to make sure, because I have not made any particular study of that feature. I may say that was not the patent I had in mind when I was referring to Adams having the first disclosure of this feature.

Q. What patent did you have in mind?

A. I had in mind the other two patents primarily in regard to that feature.

Q. The one filed in 1909 and the one filed in 1911?

[fol. 1056] A. Yes, the latter of which in some peculiar way I think was a division of the other, but I have forgotten the technical details. Those are the two I had in mind for that feature.

Q. Now, what is the distinction between the Cross and the tube and tank process?

A. The distinction in what sense?

Q. What is the difference between the two?

A. All the differences?

Q. Yes.

A. One of the most fundamental differences between the two processes is the fact that Cross emphasizes doing only incipient cracking in his coil and passing the oil through into the large reaction chamber and doing practically all the cracking there.

Also, Cross characteristically works at much higher pressures than tube and tank does, and holds the vapors in their digesting drums quite a big longer than tube and tank does,

which makes quite an appreciable change in the character of the product and particularly in the amount of gas.

Cross runs, I should say, on the average twice as long as tube and tank between cleanings, partly because of the higher pressures and partly because of the fact that he does less cracking in his coil than tube and tank.

Tube and tank, as I pointed out, do a very considerable amount of cracking in their coil and are content to clean every three to five days, in many cases, in order to get the extra yield of gasoline that accompanies that.

They have a larger proportion of liquid in their soaking drum and a lower pressure and get less gas loss but more coke loss because the composition of the stock in the still is characteristically quite a bit heavier than that in the Cross process.

Also, Cross gets comparatively little advantage in separating gas from distillate under pressure, because they use quite low pressures for that separation.

Q. Cross?

A. Yes, 10 to 20 pounds, whereas tube and tank customarily separate at about fifty pounds.

Q. That is, after it leaves the soaking drum or digesting chamber?

A. That is after it leaves the digesting chamber, yes. The composition of the cycle stock and of the tar is also different because of the considerable difference in the pressures in the [fol. 1057] separating chamber, the higher pressures being used in the tube and tank chamber.

In addition to that, there are a considerable number of differences in the construction that is customarily used. For example, in the design of the cracking coil and the flow of the oil through it and in the arrangement of the soaker vertically instead of horizontally.

Q. Those are all minor differences, are they not, and they are not essential?

A. Well, they are characteristic features of the two processes and they do produce certain differences in result.

Q. Would Cross operate any differently if the reaction chamber were set vertically?

A. Yes, it would, particularly, if it were used like a tube and tank chamber, with a liquid drawoff at the same point, and still more differently if it were operated at tube and tank

pressures, but of course, that is not what you are directing your attention to here.

Q. Of course, if it were operated and constructed just the same as tube and tank it would not be Cross?

A. No.

Q. You say in the Cross process most of the cracking takes place in the drum?

A. Yes.

Q. And all of your coke is deposited there?

A. Substantially all. There may be a little coke in the coil.

Q. You can let that coke accumulate in the drum until it is about half full before it is cleaned out?

A. You can. It is generally desirable to draw it off somewhat before it is half full.

Q. But it will work right up until it is half full without any serious results?

A. Well, you won't get as effective cracking if you are taking up a considerable volume of your chamber with coke, as if you have that available for liquid.

It is a question of economic balance when you should incur the expense of shutting down and cleaning out the coke.

It is also true that while we speak of it as being full of coke, the coke is a very porous mass and it can hold a lot of oil, even though it be full of this porous mass of coke.

Q. Does pressure have any significance in the Cross process?

A. Decidedly.

[fol. 1058] Q. What does it have?

A. It keeps the liquid considerably lighter than it would otherwise be and thus cuts down the coke deposition. Keeps it lighter by dissolving in it some of the constituents which, in a process like the tube and tank, would be vapor.

Q. In what phase are the hydrocarbons in this reaction chamber of Cross?

A. Both phases.

Q. Both liquid and gas?

A. Both liquid and vapor.

Q. And that liquid and vapor in this reaction chamber, is it about 850 degrees Fahrenheit?

A. It goes in at about 850; it leaves at quite a little lower temperature than that.

Q. What temperature?

A. As I recall it, about 775. I am not absolutely positive of that but that is the vicinity.

Q. And the pressure is about 700 pounds?

A. About 750 pounds, yes.

Q. How does the composition of the cycle stock differ in these processes; what is the difference?

A. Of course you will have to give me certain assumptions there, as to whether they are starting with the same kind of stocks and doing the same kind of cracking and things of that kind. They will vary with a great many different things.

Q. Is there any dividing line on which you can class all your Cross cracking stocks and all of the tube and tank stocks?

A. No, they will use the same stocks and frequently do use very similar stocks.

Q. Wherein is the difference then?

A. Difference in what?

Q. In the charging stock.

A. I did not say there was any difference. I said that the character of the cycle stock will vary greatly depending on the character of the charging stock which you put into them. You put different charging stocks in each one.

Q. But there is no essential difference in the stocks between Cross and tube and tank?

A. Frequently there is; sometimes not.

Q. May or may not?

A. May or may not be differences in stock.

[fol. 1059] The Master: Assuming the same charging stock, what is the difference in the cycle stock?

The Witness: Assuming the same charging stock—will, then, we will have to ask for some further assumptions as to the amount of cracking done per pass. Would you want to assume equal cracking in passing through the system?

Q. Do they get equal cracking.

A. Sometimes they do and sometimes they don't. They can get the same amount but I should say generally they do not.

Q. Then they may or may not differ?

A. Even if you have no difference in those two respects you would still get differences in your cycle stocks. That is what I want to get clear. Do you want the difference in the cycle stock, assuming that you start with the same charging stock and do the same percentage of cracking?

Q. Yes, give me that.

A. And by doing the same percentage of cracking, you mean making the same percentage of gasoline?

Q. Do they get the same percentage of cracking?

A. Sometimes they do and sometimes they don't. They can get the same percentage of cracking.

Q. I do not see how that makes any difference. Does Cross have any gasoline vapors in his vapor phase in his digesting chamber or reaction chamber?

A. I should say he did.

Q. About how much?

A. Your mean what proportion?

Q. Percentage, yes.

A. Well, I don't know. I never analyzed them.

Q. Have you any idea?

A. No, I really have not. I have never stopped to figure out. We are in a range there where we do not know much about the physical properties of the kind of hydrocarbons that Cross is handling.

Q. Would they be negligible?

A. The proportion of——

Q. The gasoline vapors?

A. Gasoline vapors in the——

Q. In the reaction chamber?

A. No, they would not be negligible; certainly not.

Q. No?

[fol. 1060] A. The vapors would contain a considerable amount of gasoline.

Q. Will you tell me again what gas oil is?

A. Are you speaking now of the primary gas oil cut or what we might call the secondary gas oil cut, which brings together the products which really belong with gas oil, but which went away with the pressing distillates and with kerosene?

A. I was speaking of the material sold as gas oil in commerce?

A. Well, the material sold as gas oil is quite a variable beast in different refineries.

Q. How does it vary?

A. Well, it varies quite a bit in character, of course, depending on the kind of crude from which it was taken, and depending whether it came from a coking operation or running for steam refined; and depending upon what the demand for lubricants was in that particular plant.

By and large, the customary gas oil of commerce, I suppose, is today the material boiling between about 450 or 500 and about 750 or a little higher.

Q. Do they get gas oil from all of the crudes produced?

A. They can get it. I do not think any considerable amounts of gas oil are made from the heavy California crudes, however.

Q. They could make it?

A. You can get a certain amount of rather undesirable gas oil. Its sulphur content would be high. It is always objectionable, but you can get material boiling in that range.

Q. Can you get it from Gulf Coast crude?

A. You can get a certain amount, though not a very high yield.

Q. And the mid-continent?

A. Yes, that gives us quite a high yield of gas oil.

Q. Pennsylvania oil?

A. Yes, you can get a gas oil cut from them.

Q. Can you get gas oil from Russian oils?

A. I presume so, although I am not personally familiar with them.

Q. Do you know how they differ from California oils?

A. No, I do not. I have never paid any particular attention to the details of the composition of Russian oils. We are tucked away in the interior and these foreign oils are not a matter of very much interest to us. I have un- [fol. 1061] doubtedly read certain statements about them but I do not recall them now. But, as I recall it, they are rather highly asphaltic.

Q. Can you define gas oil as being any oil which boils about five hundred degrees Fahrenheit?

A. No, that definition would be broader than gas oil.

Q. It would include residuum would it?

A. Any oil that boils above 500?

Q. Yes.

A. Yes, it would.

Q. And lubricating stocks.

A. Yes.

Q. Would you define gas oil as being an intermediate oil?

A. What do you mean by "an intermediate oil?"

Q. Well, it is intermediate between kerosene and the heavier stocks, is it not?

A. Its boiling point and gravity is intermediate between that of kerosene and heavier stocks, yes.

Q. Now, how does gas oil differ from fuel oil?

A. Well, fuel oil is, today particularly, a much broader term. We have really two kinds of fuel oils; what we call distillate fuel oil and residual fuel oil. Distillate fuel oils are characteristically used for small burners for household use, and the residual fuel oils, which are quite a different type of material, and are only made by plants running on the skimming process.

Q. Did they have use for any distillate fuel oils in 1912?

A. Yes.

Q. And they also used the residual fuel oils?

A. Those refineries that were running by the skimming method of operation made residual fuel oils. Of course, you get no residual fuel oils when you are running to coke or by any process which makes lubricants. Excuse me, that statement may be just a little too broad. By any of the common processes for making lubricants you do not get any residual fuel oils; you only get the distillate fuel oil, which is the gas oil or part thereof.

Q. But by some of the lubricating processes you do get a residual fuel oil?

A. Well, I don't know of any plant operated that way, but I realize that it would be possible to conduct a rather peculiar type of operation which would take off part of the lubricants and leave the others in the residuum and then [fol. 1062] sell that as a fuel oil. I presume that is being done in some places today, although not in any of our refineries.

Q. But fuel oil and gas oil are not always the same thing?

A. No, although distillate fuel oil and gas oil would be almost synonymous terms.

Q. But the distillate fuel oil might contain quite a lot of heavier materials?

A. No, because you went to the trouble to distill out the heavier materials you would use them for lubricants, if you are going to sell them as fuel oil why bother to distill them?

Q. What is motor spirits?

A. A wide variety of things.

Q. Might cover anything, might be used——

A. If you do not have any context to go with it, why, it might be almost anything. It has been applied to a wide variety of different materials.

Q. It includes gasoline, does it?

A. Well, it is not ordinarily applied to gasoline but, yes, it includes gasoline.

Q. What is the difference between naphtha and gasoline; is there any difference?

A. Well, gasoline is simply one of many naphthas which are available at any refinery.

Q. Naphtha is a broader term than gasoline?

A. Naphtha is a broader term than gasoline.

Q. In some of the art the word "essence" appears. Is that the same as gasoline?

A. Well, it might or might not be. I should say in general not. The term "essence" is not a thing that we use in this country at all commonly.

Q. It is used in France, is it not?

A. Yes, I think it is.

Q. What does it indicate over in France, do you know?

A. I think it means the very light distillate like our——

Q. Rhigolene?

A. That was not the name I had in mind. Not quite as light as our petroleum ether. I think it is that type of a product but I am not positive.

Q. What is a mineral spirit; is that the same as gasoline?

A. Mineral spirit? Its use in this country or—of course, in these foreign usages you can find most anything you want.

Q. I think it is used in this country.

A. The only thing that I can think of that is sometimes [fol. 1063] called mineral spirits is what we call painters' and varnish makers' naphtha.

Q. That is lighter than kerosene, is it not?

A. That is usually lighter than kerosene, yes; although I have seen some kerosenes which are almost as light as that, but usually it is lighter than the ordinary kerosene, yes.

Q. It is somewhat the same as gasoline?

A. No, it would not be a satisfactory gasoline because it lacks the light ends as a rule. They generally have some flash point requirement on that.

Q. But there may be light mineral spirits that are much the same as gasoline?

A. In the only place that I have heard the term used, it applies to materials which, as I recall the distillation, would not be suitable for gasoline.

Our varnish makers' and painters' naphtha is an intermediate cut out of about the middle of gasoline, but does not contain enough light ends to be a satisfactory gasoline, and that is the only thing that I recall that is ordinarily called mineral spirits.

Q. You do not know whether that was a lighter material when the kerosene was more valuable?

A. No, I don't think kerosene ever got down much into the range of V. M. & P. It might have been lighter for other reasons at earlier times, but I do not think the question of how much kerosene you are making would have much effect on the boiling point of V. M. and P.

Q. It was always lighter than kerosene, was it not; a lighter cut?

A. If, when this term "mineral spirits" was used it referred to Varnish Makers' and Painters' naphtha, I should say that it was.

Q. Was gasoline ever used in making gas?

A. What kind of gasoline?

Q. The ordinary type, light gasoline?

A. Well, the ordinary type is not light gasoline.

Q. Benzine, for instance; ordinarily sold years ago, as benzine?

A. Yes, for some very light benzines, but they were not modern gasoline by any means, but very light products, what I would call gas machine gasoline.

Q. That is just soaked up in the gases as they pass?

A. In the air.

[fol. 1064] Q. Was it ever used for enriching coal gas?

A. For enriching coal gas?

Q. Yes.

A. I don't know.

Q. It could have been used for that, could it not?

A. A little bit of the very lightest ends could have been. You would not want to put in the heavier ends to any appreciable extent, because they would condense out in the lines when it got cold; but the very lightest ends of gasoline could go in.

Q. What are the essential features of the Burton process Patent No. 1,049,667?

A. Just what do you mean by "essential"?

Q. I understood from your direct examination that the essential features were the use of the clean stock, gas oil, and correlation of temperature and pressure; is that correct?

A. Combined with the feature of pressure condensation which increases the quantity and improves the quality of the distillate.

Q. That term "correlation of temperature and pressure" is not quite clear to me. Will you explain what it means?

A. Why, it means simply this: that in a bulk pressure distillation process for any given stock, if you wish to make a given product, you must control the pressure within quite narrow limits to accomplish that desired purpose.

Say, for example, that you decide a temperature of 750 degrees is a suitable operating temperature. For this temperature there is a certain pressure which will allow a product which is largely, but not entirely gasoline, to escape and yet not keep a great deal of gasoline back in the still. If you use a pressure which is higher than that it will keep too much gasoline in the still, thus producing additional cracking, destroying some gasoline and making additional quantities of gas.

If, on the other hand, we use too low a pressure it allows a product to distill out of the still which carries large amounts of kerosene, possibly even some gas oil, along with it, and so gives us a distillate which is much poorer

in quality than the desired distillate, and prevents us from cracking the material which we want to crack.

Now, the correlation that I am speaking of is getting the pressure which is to go with a suitable temperature to give you a certain desired product without too much gas loss. [fol. 1065] Q. That is, it is quite essential to know the pressure to use then?

A. Within fairly close limits.

The Master: And where, in the Burton patent, No. 1049667, is that pointed out?

The Witness: Burton's preferred operating conditions are given as 750 to 800 degrees still temperature, and 4 to 5 atmospheres pressure, this being given on page 2, lines 17 and 21, respectively. As I pointed out, we have slightly changed that correlation, from what it was then to accom--odate slight changes in charging stock.

The Master: Is the object of this correlation of pressure and temperature, stated on the first page of the patent, line 79?

The Witness: Yes. The first part of that statement is, at least, part of the object of this correlation.

Q. Is the use of the run-back essential to this Burton patent?

A. It is not essential, no.

Q. It will operate successfully without it, will it?

A. Yes, but it is greatly improved by the addition of the run-back, of course.

Q. Did I understand, from one of your answers, that gasoline will crack more easily than kerosene?

A. No, I didn't make any such statement as that.

Q. Do you know whether it will, or not?

A. As I recall it, gasoline from mid-continent crude will not crack quite as readily as kerosene, although the difference is not extremely large.

Q. Is kerosene harder to crack than gas oil—that is, does it take a higher temperature?

A. Now, maybe we had better be sure what you mean by harder to crack.

Q. I mean is a higher temperature required?

A. It will all begin to crack at about the same temperature. All we can say is that one part of it will crack a little more rapidly than another part.

Q. Does this rate of cracking increase with the molecular weight, or decrease?

A. It is not constant. It varies with the stock, and, as I recall it, our results indicate that gas oil cracks a little more rapidly at a given temperature, than the materials [fol. 1066] on either side, but I am not sure. I know there is a maximum some place in that vicinity.

Q. So that kerosene cracks at a higher temperature, or at a lower rate at the same temperature?

A. Kerosene will crack at a little lower rate at the same temperature, than gas oil, but the difference is not large—at least, with mid-continent stock. Of course, outside of that I don't know how they compare. Accurate determinations are quite difficult.

Q. Which of the defendants use this Burton process?

A. Of the primary defendants?

Q. Yes.

A. The Standard Oil Company of Indiana, and the Standard Oil Company of New Jersey.

Q. It is not used in the Holmes-Manley, or the Cross processes?

A. What do you mean by the Burton process? Do you mean as I described the process, as a whole?

Q. The essential features, as you have given them.

A. Oh, that is quite a different matter. The essential features, the selection of the stock and the correlation of temperature and pressure, and——

Q. As given by Burton?

A. The narrow figures, as given by Burton, are, I believe, only used in the Burton process, but the broader aspect, of course, is a different matter.

I believe the Holmes-Manley process has been operated under these particular correlations, but they preferred to increase both temperature and pressure slightly, to get a faster cracking rate, now that stronger stills are available.

Q. But it is your position that the patent to Young does not disclose the use of gas oil?

A. Young?

Q. Yes—the British patent, "Exhibit No. 93"?

A. I certainly never have seen any indication that he did. Is that "Exhibit No. 93" that you refer to?

Q. Yes.

A. Yes, that is right.

The Master: Recurring, again to this Burton process—does the Holmes-Manley process do this: Raise the boiling point of the liquid residue and increase the heat influence thereon, while undergoing distillation, by maintaining that pressure on the liquid, and the vapors arising therefrom, [fol. 1067] by distillation, and also maintaining the vapors, themselves, under pressure throughout their course from the still, through the condenser, while undergoing condensation?

The Witness: Yes, they do—they do do that.

Just what do you refer to as indicating any possible use of gas oil, Mr. Rauber?

Q. I note, at the top of page 1257 of the Book of Exhibits, that he states his invention applies to both heavy and light oils. Wouldn't a light oil be gas oil, when you use it as a charging stock?

A. Not made from shale oil, which is evidently what he is concerned with, in this part of his discussion.

Q. Wasn't petroleum known in 1865?

A. Yes.

Q. He mentions petroleum, at the bottom of page 1256.

A. But, as plaintiff's expert pointed out, petroleum was a term commonly used in Europe for shale oil.

Q. And also for American petroleum?

A. American petroleum was only discovered in 1864, as I recall it.

Q. 1859, I think.

A. Well, I doubt very much if James Young was giving much attention to the minor importance of American petroleum at that date.

Q. But he did use the light distillate, did he not?

A. Well, of course all shale oil is, in a sense, a distillate, and he speaks, here, of the process of re-running shale oil to give once run shale oil, as I recall it—so that the original shale oil is a distillate, and once run shale is another distillate.

Q. Now, at the bottom of page 1257, he states—about ten lines up:

"Or the crude oil may be first distilled in the usual manner, and the products of such distillation may be treated according to my invention."

Would not such heavy distillate be within the range of gas oil?

A. But it does not bear the remotest resemblance to Burton's gas oil. It might or might not have the boiling points but it is far from being the same, or a similar product.

Q. To what gas oils does Burton's patent refer?

A. To the gas oil made from petroleum.

Q. What kind of petroleum—any particular kind.

A. He doesn't specify any particular kind.

[fol. 1068] Q. It might be from any source?

A. I should say it would have to be a petroleum that was capable of making lubricants and wax, because he definitely describes his gas oil as being that.

Of course, you might find other stocks which were equivalent, and which were made from other petroleums. But he does definitely disclose that his gas oil is from a petroleum which makes lubricants and wax.

Q. How is anyone to determine whether the gas oil that they have is the one specified in the Burton patent?

A. Well, that is a legal matter of equivalents. You can compare it in various way, in the laboratory, and in other ways. You might have to compare it in a pressure still.

Q. And then Burton's patent simply covers whatever is useful in making gasoline?

A. Hardly. Burton discloses a particular stock which is very useful in making gasoline.

The Master: Which patent are you referring to?

Mr. Rauber: No. 1049667.

The Master: You are still referring to that?

Mr. Rauber: Yes, sir.

The Mas-er: That patent was for the treating of a particular stock—Burton's?

The Witness: Yes, sir.

The Master: In a particular manner?

The Witness: Yes, sir.

Q. And that stock was gas oil?

A. That stock was gas oil.

Q. Will you please point out, in the Burton patent, No. 1049667, being "Exhibit No. 176," wherein he defines this gas oil? He mentions, on line 25, fuel oil, does he not?

A. Yes.

Q. That is not a gas oil, is it?

A. That is a distillate fuel oil, substantially the same as gas oil.

Q. There are other fuel oils which are not gas oils?

A. As I said, distillate fuel oil is substantially gas oil, and it is only fuel oil which is made when you are making lubricants and wax.

Q. But not all fuel oils are gas oils?

A. No—only distillate fuel oils.

Q. There is nothing stated in this patent about gas oil [fol. 1069] from different sources—whether they may be from California, or Russia?

A. No, he doesn't say specifically, except that he does make clear that he made his gas oil from a crude that contained lubricants and wax.

Q. And you may get residual fuel oils in the process of making lubricants?

A. I say it is done, but not from ordinary processes. I don't know of its being done before Burton's time.

Q. Bnt any crude oil, or heavy fuel oil would boil above 500, would it?

A. It would boil above 500.

Q. Then, any oil boiling from 500 up, would not necessarily be gas oil, would it?

A. No. With that, only, as the description, it would not necessarily be a gas oil.

You didn't give me chance to answer the question you asked. Do you mean to withdraw that question about where he discloses his gas oil?

Q. Yes, I think I brought out the point I wanted. Now, with reference to the prior Riebeck patent—did I understand you to say that he did not use a gas oil?

A. I said that what he called gas oil was an entirely different thing from what Burton called gas oil.

Q. But it was a gas oil, was it not?

A. It was a gas oil, used in making gas, yes.

Q. And it was an intermediate distillate?

A. No, I don't think it was. It was his heavy dark paraffin oils from brown coal tar. I think that was the heaviest part of the brown coal tar distillate.

Q. In what manner do they differ from the ordinary American gas oils?

A. In every manner—in boiling point and dirtiness.

Q. What is their boiling point range?

A. I don't know, but they are evidently quote heavy viscous oils, and I don't think there is any doubt but what they boil quite a bit higher than ordinary gas oil.

Q. About 500 degrees Fahrenheit?

A. Above 500, yes.

Q. They also made lubricants, didn't they, from those oils?

A. Yes, sir, they made lubricants.

Q. And they made waxes

A. I don't know whether they made wax from the brown [fol. 1070] coal tar, or not. I guess they did make a particular kind of wax—not paraffin wax, but I think they did make a certain brown type of wax from some of those oils.

Q. So you get your lighter products and you get your wax and lubricating oils and gas oils, and different products—is that right?

A. Well, it is such an entirely different process that there is hardly any resemblance, at all.

Q. It is a distillation, isn't it?

A. Yes. You distill brown coal to make these tarry oils, and in some cases you redistill the tarry oils.

Q. And that is true of Baku petroleums, from Russia, is it not?

A. Just what is true?

Q. That they get gas oils and lubricants and waxes from them?

A. I don't know whether they do, or not. I don't know enough about Russian petroleum to state.

Q. The gas oil would be in the same relative proportion if they got it—I mean, the same relation to the other products?

A. Well, if Baku crude is like our crude, why, yes. If it is not, I cannot predict, and I, unfortunately do not know just what the characteristics of those crude petroleums are.

Q. But if you found that Krey used a gas oil from Baku petroleum, it would probably be the same as the American, would it not?

A. What they use for gas oil, in Europe is apparently quite different from what we use as gas oil here. That is, as oils for making gas. Those heavy coal tar products would never be considered as gas oils, here.

Q. Why not?

A. Because they are an entirely different type of product, which would not meet the specifications—they are much higher boiling, for one thing.

Q. What harm would they do in gas making?

A. Well, they have a lot of specifications worked out for different purposes.

Q. What is the purpose of the light distillates? Why can't they use heavy ones?

A. I am not certain, but I think, for one reason, that—well, I don't know, and I had better not guess. I do know they have specifications which very definitely limit the amount of material boiling higher than 750 degrees, be- [fol. 1071] cause they make trouble one way or the other. But it may be simply that they don't get as good a yield of gas, as from others.

Q. You don't know what kind of troubles they make?

A. I don't know.

Q. Whether it is coke?

A. I don't know—I was going to guess that, but I don't know.

Q. The probability is that they get too much coke?

A. From some very heavy stocks they probably would get too much coke.

Q. The disadvantage of getting dirty products would depreciate it in the gas industry, would it not?

A. I don't know. There is no question but what in flash cracking excessive coke is an undesirable thing.

Q. And that is what gas oil has been used for?

A. In the flash cracking industry, yes—it has been extensively used there.

Q. So that the desirability of using a light distillate—a non-dirty distillate—was appreciated?

A. I don't know of any evidence that it was. I presume that is one reason why they didn't use the same kind of stocks in this country, as they apparently used in Europe. But where they could choose they preferred the lighter stocks.

Q. What makes you think they used heavy stocks over in Europe?

A. Because Riebeck says these heavy, dark, paraffin oils were used for gas making purposes.

Q. That would describe any oil heavier than kerosene, wouldn't it? Relatively it would be a dark, heavy oil?

A. Well, it wouldn't be a dark, heavy paraffin oil, and he describes the viscosity and density of some of them, as I recall.

Q. From which gas oil is distilled?

A. No—of the heavy, dark paraffin oils, themselves.

Q. He speaks of a light gas oil, too, on page 4212—the fourth line from the bottom?

A. Yes, he speaks of light gas oils. He speaks of them as being saleable products, as distinguished from the dark gas oils. He was mainly interested in getting a ready market for the dark gas oils, which were not as readily saleable as the lighter oils.

Q. Then, there is no question but what he used the German gas oil in his process, is there?

[fol.1072] A. What do you mean by the German gas oil?

Q. The gas oils made at the Webau plant.

A. What he cracked was the dark oils. He was apparently satisfied with the market for these light gas oils.

Q. Now, the presence of creosote—what influence has that on that kind of dirty stock?

A. Do you mean, is creosote a cleaner or a dirtier stock?

Q. Would the presence of creosote in a gas oil have any effect?

A. On what?

Q. On the cracking?

A. Yes, sure.

Q. What effect would it have?

A. I don't know.

Q. How do you know it would have any?

A. Because any new chemical compound which you put in breaks up differently from the old chemical compound.

Q. Would it crack as readily?

A. No, it would fail to crack as readily. That would be a difference.

Q. It would be simply an inert material, would it not?

A. I am quite sure that creosote would not crack as readily from a general knowledge of chemistry—although I have never worked with it, particularly.

Q. That is just from theorizing, you assume that?

A. Yes.

Q. Do you know whether the process, such as Riebeck describes, would yield gasoline, or not?

A. Yes, undoubtedly. Of course he describes a lot of dif-

ferent processe, but they would undoubtedly yield small amounts of gasoline.

Q. How much?

A. Oh, it would be entirely dependent on what he started with, and how he cracked, and whether he went down to coke, or not. He gives, in his preferred operation, a direction statement of how much he made.

Q. How much was that?

A. As I recall it, it was six parts of benzine to 33 parts of illuminating oil, from a hundred parts of stock.

Q. That might not be the same type of gasoline that they have today—it was probably lighter, wasn't it?

A. It probably was.

Q. If he classed as gasoline everything that boiled below [fol. 1073] 200 degrees Centigrade, that would be the modern type of gasoline, would it?

A. No.

Q. Modern is below 225 degrees C., I believe.

A. That is the end point. But as I pointed out we can't put into gasoline everything that boils below the end point. We try to but we can't put it all in. As I said, there is quite a bit below 400 degrees F. which we cannot put in and meet the specifications for gasoline.

Q. But in running your stills where do you begin to cut? Around 400 degrees?

A. That depends on how much steam we are using.

Q. You don't use steam in your stills, do you?

A. Not in the coking stills. We don't cut by temperature—we cut by gravity, as a rule.

Q. When you don't use steam you cut around 400?

A. We don't cut by temperature. We can probably figure it out, but I think it would be lower than that. I think that if we cut at 400 in an ordinary still, with fractioning, we would have material in the distillate boiling as high as 450, without any question.

Q. I note that on page 4239, the description of the analysis of the Riebeck product is given, and he lists all of those up to 200 degrees. Would those products, boiling below 200, be gasoline? That is, would there be any gasoline cut?

A. There may, or may not. Gasoline is a thing that has a certain proportion of the different constituents. If you are going to make gasoline by cracking, you have to

have the right proportion of light ends, the intermediate parts and the heavy ends. You can't just say that everything below a certain point. As I say, we can't actually get within 50 degrees of our theoretical end point, and put all the material in. We have got to put part of it in kerosene.

The Master: Reasoning from the process disclosed by Riebeck, and the products described, would you say that they contained the distillates belonging to the objectionable ethylene group?

The Witness: That would depend a good bit on the kind of stock you were using.

The Master: Would it be undesirable by reason of the offensive odor?

The Witness: It certainly would have the offensive odor, [fol. 1074] all right. These brown, coal tar products are all very vile in odor, and are very difficult to refine.

Q. I note, on page 261 of Bulletin 17 of the Kansas City Testing Laboratory, the following statement: That crude benzine, gasoline and naphtha includes all of the light distillates which vaporize up to 410 degrees Fahrenheit.

A. That is true, you can put into the naphthas what won't go into the gasoline. That is prefectly true.

Q. Do you think that everything that boils up in the still, like that—that is, boils up to 410 degrees Fahrenheit—would be classed as gasoline?

A. I am quite sure it could not. It would go into either gasoline or naphthas. If you had a market for all the heavy naphtha and oleum spirits, and things of that kind, you could get rid of it.

Q. It would go into naphtha and gasoline?

A. It would go into naphtha or gasoline. It could go into naphtha or gasoline. Generally, our market for naphtha is not sufficient to take care of all that, and we have to put quite a bit of it in kerosene.

Q. This 200 degree point, in this table given on page 4239—that is somewhat below 410, is it not?

A. Yes.

Q. Now, in the United States Patent to Dewar & Redwood, No. 419,931—

A. What exhibit?

Q. Petitioner's Exhibit 103, we have a process in which oil is distilled and condensed under pressure, do we not?

A. Yes.

Q. That is the same method of operation that Burton has, is it not?

A. In what respects?

Q. In the distillation and condensing under pressure.

A. It is the same in the fact that they do distill and condense under pressure, although not exactly in the means by which they accomplish that.

Q. You mean the construction of the still is a little different?

A. No, I mean Dewar & Redwood disclose the pumping in of air or other gas to maintain the desired pressure. Of course, Burton—

Q. You mean that is pumped in all the time throughout the process?

[fol. 1075] A. No, I judge from the description that it is pumped in most of the time; not all of the time.

Q. Pumped in at the start?

A. Undoubtedly pumped in at the start and pumped in all other times except during those occasional periods he mentions where an excess of gas is being made, and I assume a sensible man would stop the pumping if he finds he is making an excess of gas, which they do at times.

Q. So if you felt sure that Dewar & Redwood was always making an excess of gas, you would feel sure he was not pumping any gas after they started?

A. Yes.

Q. Is there any other differences between Dewar & Redwood and Burton?

A. Yes, many, many others; for instance the kind of stock they crack.

Q. What kind of stock do Dewar & Redwood crack?

A. It is reduced crude. Do you want that line reference?

Q. Yes.

A. A reduced crude as indicated in line 25 of page 1, "Residue after distilling off the illuminating oil from crude oils."

Q. That would be classed as a fuel oil, would it not?

A. It might be sold as a fuel oil and probably could have been sold as a fuel oil.

Q. It would be an oil boiling upwards of five hundred degrees Fahrenheit?

A. It would boil above five hundred degrees Fahrenheit.

Q. Upwards of five hundred degrees Fahrenheit?

A. I do not want you to read anything into that "upwards of". I think "upwards of" may very properly be considered to have the significance of above but in the general vicinity of five hundred. I think it has a little different meaning from "above."

Q. If it boils upwards of, it must be above?

A. Yes, but if it is boiling above he would not necessarily say it was upwards of. I mean "upwards of" means in that general vicinity, but—

Q. Boils above?

A. It is above in that general vicinity. Whereas, in the case of crude oil, the "above" would go clear up to ten or twelve hundred degrees, if we could boil at those tem- [fol. 1076] peratures without cracking. That is, the true boiling points would be in that vicinity.

Q. Is there any other distinction between Dewar & Redwood and Burton?

A. In the product they are making.

Q. What product is Dewar & Redwood making?

A. Illuminating oil.

Q. And gasoline?

A. Oh, he would inevitably get a certain amount of gasoline along with his illuminating oil, yes.

Q. How much?

A. That depends on his operating conditions. I should say, as a rule, if we are cracking to make kerosene and have such a correlation of temperature and pressure as to be suitable for making kerosene, that is, not to recrack any more kerosene than necessary, which of course Dewar & Redwood do not disclose, but if they——

Q. What temperature and pressure would they need for kerosene?

A. I really don't know. It is a thing we never really try to make in our pressure stills. It is undoubtedly a much lower pressure for the temperature, or a much higher

temperature for the pressure than that required to make gasoline from a given stock.

Q. This same process is described on pages 1388 and 1389 of the book of exhibits, Exhibit No. 107, is it not?

A. Essentially the same, yes, as I recall it.

Q. On page 1389, near the bottom of the page he states that "Intermediate and heavy oils may be readily converted;" to a considerable extent." That intermediate oil would correspond to a gas oil, would it not?

A. Not necessarily.

Q. It is intermediate between kerosene and some other heavier product?

A. Yes, but it is far from being the only intermediate oil.

Q. You do not use simply gas oil in the Burton still, do you?

A. Yes, except for cycle stock and for naphtha bottoms which are part of the secondary gas oil cut.

Q. Don't you use some paraffin distillate?

A. No. Some refineries do but we do not.

Q. But you can use it?

[fol. 1077] A. You can use a certain proportion of paraffin distillate along with the others; yes.

Q. That is an intermediate oil?

A. Intermediate between what?

Q. An intermediate oil between the light and the heavy?

A. Yes, intermediate between light oils and heavy oils.

Q. Further on, on that same page, Dewar & Redwood say: "May be readily converted to a considerable extent into lighter oils suitable for use in ordinary mineral oil lamps and even mineral spirit of low boiling point." That mineral spirit would be something lighter than kerosene, would it not?

A. I should judge so, especially when he says, "capable of being—" well, now wait. "Carburetting coal gas". Of course, you employ gas oil for that, which is heavier than kerosene.

Q. Yes, but you said it might be used in carbuertting in the ordinary sense, like you do for making air gas?

A. Yes.

Q. In that case it would contain some very light ends, would it not?

A. Yes. I should not be surprised if this referred to the lighter material below the kerosene range which he made in his process.

Q. Especially, as he refers to it as mineral spirit?

A. Well, that would not be our mineral spirit, certainly.

Q. No, it would be something better?

A. Our mineral spirit would be quite a different product from that.

Q. Mineral spirit is the cut lighter than kerosene ordinarily, is it not?

A. As we in the United States use the term, yes.

Q. And kerosene in those days had a lower initial than it has now?

A. Yes, as a rule. It largely depends on what "carburetting" refers to.

Q. Further up in that same page Dewar & Redwood state that by regulating the heat and pressure to which the retort is subjected the character of the distillate may be varied. Does that not refer to a correlation of temperature and pressure?

A. Well, he is simply making an obvious statement, that anyone would know. He is not giving the correlation; he is not helping you to solve your problem.

[fol. 1078] Q. So it is necessary to state both temperature and pressure?

A. If you are stating a correlation, it includes both temperature and pressure.

Q. If Burton had not given the pressure, why, then they would have failed to disclose this correlation, would they not?

A. I should say so, yes. The correlation of temperature and pressure means specifying a certain temperature for a certain pressure for a given product.

Q. Do you know that they failed to state the pressure in their applications as filed?

A. No.

The Master: What exhibit number is it?

Mr. Rauber: Exhibit 107, it is page 1389. That is the Dewar and Redwood process.

Q. Do you think that anyone skilled in the art would understand that in the Burton patent only gas oil was used?

A. Those skilled in the art?

Q. Yes.

A. Yes. Certainly, anyone skilled in the art and reading the patent carefully could not arrive at any other conclusion. I would not say there may not have been someone who read it hastily who might not get that impression.

Q. Do you know whether Humphreys in his patent No. 1,119,700 (that is the runback patent, Exhibit 202) mentions gas oil?

A. No, I do not recall whether he does or not. Of course, that is directing to returning the reflux and not to the cracking of a particular stock. Do you want me to look at it?

Q. On page 1935 he speaks of it as fuel oil, as charging stock.

A. Of course, he refers in this to the Burton patent. He refers clearly to the fact that he is practicing the Burton invention. He does not go into the details which Burton had already disclosed.

Q. He mentions an oil which might be reduced crude?

A. Where is that?

Q. Fuel oil.

A. Where is that, in what line?

Q. Line 14, page 1935.

A. The mere term "fuel oil", if there were no reference [fol. 1079] to the Burton patent or something else would include both products, and of course Humphreys' runback is applicable regardless of the type of product that is being cracked. The run-back principle is a separate thing. Of course, Burton had the other invention which was the stock.

Q. That run-back returns everything higher than gasoline?

A. Not entirely.

Q. Returns much gasoline?

A. Returns everything but the desired product; it depends how much kerosene you want to make.

Q. Does it return any gasoline?

A. It undoubtedly condenses a certain amount of gasoline but practicaly all of that is stripped out in returning the condensate counter-current to the hot vapors before it reaches the still. I presume there may be a trace of the heavy ends of gasoline still in it. It certainly does not return any gasoline as such.

Q. And that return amounts to about ninety per cent of the vapors?

A. Eighty to ninety per cent of the vapors leaving the still.

Q. So, without the run-back, only about ten to twenty per cent of Burton's vapors would be gasoline?

A. Oh, no. That depends on your operating conditions. When you put the run-back on you make a slight change in the proper temperature and pressure for operating the still The run-back enables you to get a higher still temperature for a given pressure and yet get a good distillate. In other words, in effect it tends to increase the rate of cracking.

Q. That temperature is about 720?

A. What temperature?

Q. Of cracking in the Burton still.

A. With our present stocks it starts out at about 725 and finishes up about 755.

Q. You would not say that was upwards of 700?

A. Yes, that is upwards of 700.

Q. And upwards of 650?

A. Well, it is certainly above 650.

Q. By "upwards" you mean "above" it? You said "upwards of 650".

A. I do not think "Upwards" includes from here on, like above does. Even 725 to 755 is not very far above 650, although it is not immediately contiguous thereto.

[fol. 1080] Q. Do you know of any of the other patents in the Burton group that mentioned gas oil?

A. What do you mean "the Burton group"?

Q. Burton and Humphreys and Clark.

A. The Indiana patents?

Q. The Indiana patents.

A. I do not know whether they do or do not. They are building on Burton's discovery. They frequently refer to

Burton's patents, but I do not know whether they go back and describe the stock in detail or not.

Q. Do you know whether Dewar & Redwood would make gas enough to maintain a pressure?

A. They would make gas enough to maintain some pressure, yes.

Q. I mean the required pressure for making gasoline?

A. Well, there is no particular required pressure for making gasoline. You can make gasoline without pressure entirely.

Q. Without pressure in a pressure still?

A. You can even made it at one pound pressure in a pressure still, if you want to. That is, you cannot talk about any pressure necessary for gasoline, because you can make it without pressure. You can make it in a vacuum.

Q. In all of which cases you would have enough gas to supply the pressure you desired?

A. At the very low pressures you certainly would.

Q. And at the higher pressures also?

A. At the higher pressures you would not.

Q. Fifty pounds?

A. Well, fifty pounds is quite near the border-line. I found it would not hold sixty, making kerosene, as I recall it.

Q. Would Burton's hold sixty?

A. Would Burton's hold sixty?

Q. Yes, his original disclosures, would that hold sixty pounds pressure?

A. Cracking for gasoline?

Q. Yes.

A. Yes. Not at all times. You may have to back the liquid up in the condenser in starting without the Clark high line.

Q. If you start off with gas pressure it would hold it?

A. If you start off with a gas pressure and were making gasoline distillate, such as he discloses, in general you would have enough gas to saturate the distillate.

[fol. 1081] Q. That is true of Dewar & Redwood, is it not?

A. No.

Q. If they made gasoline?

A. If they were running for making gasoline only.

Q. It wouldn't make any difference what they were running for, they would get gasoline, would then not?

A. Yes, but it is a question of whether you are taking his distillate as only substantially kerosene, or whether you take

his distillate as gasoline. That makes a big difference in the volume of the distillate and the ratio of gas to distillate. That is, of course, the important thing.

Q. Burton makes some gasoline distillate in his process, doesn't he?

A. Yes.

Q. About how much?

A. Well, the distillate generally runs about sixty per cent gasoline.

Q. Forty per cent of kerosene?

A. Forty per cent of kerosene and naphtha bottoms. You would have to leave a little bottom after the treating process, as a rule; probably you would get sixty per cent gasoline, thirty kerosene and ten of naphtha bottoms or something of that kind.

Q. Why would not Dewar & Redwood get enough gas to maintain their pressure?

A. Because their gas distillate ratio is lower than that of Burton's, because they are trying to make kerosene, and therefore the volume of the distillate is greater for a given amount of cracking. Furthermore, they probably actually do less cracking because less cracking is required to make kerosene. It is when you re-crack some of the kerosene to make gasoline, that you make a considerable volume of gas and that also reduces the amount of your distillate.

Q. You know they made some gasoline?

A. Certainly.

Q. You do not know how much?

A. No. I know that if you are cracking to produce kerosene, and therefore, try to get the kerosene out of the still as fast as it is formed, that the ratio of kerosene to gasoline is quite high; several times as much kerosene as gasoline. Whereas, Burton has only about half as much kerosene as gasoline.

Q. At what temperatures and pressures do you run to make your kerosene?

[fol. 1082] A. I don't know. I have never operated bulk pressure stills to make kerosene. As I indicated, however, the pressure would be lower for a given temperature, or the temperature would be higher for a given pressure; but you cannot talk about the pressure alone. It is correlation in this type of process that you have go to talk about.

Q. And also the lower pressure would not require as much gas to saturate the distillate would it?

A. Yes.

The Master: Did Dewar & Redwood maintain a pressure of from four to five atmospheres on their condenser?

The Witness: They do not say anything about what pressure they maintain, your Honor. They give no correlation of temperature and pressure whatever. As I indicated, running—

The Master: If practiced subsequent to 1912 and Dewar & Redwood did not carry a pressure of four to five atmospheres on their condenser, would they infringe?

The Witness: Excuse me. If Dewar & Redwood——

The Master: Did not carry a pressure of four to five atmospheres——

The Witness: I should say that Dewar & Redwood——

The Master (continued): on the condenser.

The Witness (continued): Would not infringe at all using that kind of stock, regardless of what pressure they employed. Of course, that depends to some extent on the interpretation of claims but they certainly do not have the essence of the Burton invention.

The Master: They would not lack the elements of the claims, would they not?

The Witness: They would lack the elements of the claims. The first important step was the disclosure of a clean stock which would give a black bottom in the pressure still, so that the particular pressure is not a matter of moment.

Q. And the only basis on which you base that charging stock being gas oil, is that it was upwards of five hundred degrees Fahrenheit?

A. Oh, no, not at all.

Q. That is the definition of the claim?

A. You mean Burton?

Q. Yes.

A. Well, there is no question but what Burton is disclos- [fol. 1083] ing a gas oil stock boiling mainly between six and seven hundred degrees.

Q. Or fuel oil?

A. Yes, which is distillate fuel oil?

Q. Of any kind?

A. No, it is the fuel oil which is made when you are making lubricants and wax, and that is only one kind of fuel oil, distillate fuel oil

Q. That is an intermediate fuel oil, is it not, between the kerosene and the heavier distillates?

A. Yes, it is an intermediate fuel oil between kerosene and the heavier distillates.

Q. That was the type that Dewar & Redwood described, an intermediate oil?

A. No.

Q. They mentioned an intermediate oil?

A. They mentioned intermediate or a heavy oil. If you have a crude oil which is light, and you take off gasoline and kerosene entirely, you have a heavy residue. Now, if you leave in some of the kerosene you would get an intermediate oil. In other words, it would still be a reduced crude but it would not be reduced so far. It would be the difference between the two samples of reduced crude which I showed here. In other words, I showed a sample of the reduced crude and the second reduced crude and the first of these would be an intermediate oil which had just the gasoline and kerosene taken off.

Q. You call that "intermediate"?

A. That is intermediate, certainly; between those two extremes.

Q. If you are going to make kerosene by cracking you would not leave in any, would you?

A. Probably not. It would do not particular harm, though, if he was operating at the proper temperature to make kerosene, since it would distill out in the early stages of heating up. Just as in our case, it would not matter if we put gasoline into our charging stock, because if we have the proper correlation of temperature and pressure to distill out gasoline, it would come out before we reached a cracking temperature.

The Master: The distinction you are trying to draw between the general classification of intermediate oil and Burton's oil is the definition of the difference between genus and species?

The Witness: You have an intermediate oil in a certain series, which is one thing.

The Master: Dewar & Redwood are talking about dogs, and you are talking about Airedales?

The Witness: Yes.

Q. Mr. Wilson, in respect to the use of gas oil or clean stock of the type that you use in the Burton still, is the Holmes-Manley process limited to that type or the same type of charging stock?

A. Substantially so. Of course, as I have indicated, you can run for a very short time without doing very much cracking with quite a bit heavier stocks, but it is almost always operated on Burton's type of stock for very much the same reason.

Q. You can operate it commercially on heavier stocks; is that right?

A. Well, it depends on what you mean by "commercially"? You can operate a commercial size still for a short time. I doubt if you could show any profit on the operation. If you did, it would be much less than the profit by working on Burton's stock.

Q. Do you know whether any company is operating the Holmes-Manley process on dirtier stocks than Burton uses?

A. They might be very slightly dirtier, including a little larger proportion of Chamberlain's disclosure, of adding [fol. 1084] paraffin distillate. I do not think any of them are using stocks that even go as far as Chamberlain indicated was possible in the Burton process.

Q. Where have you seen the Holmes-Manley process in operation?

A. Only in our plant at Whiting, where it is modified in certain respects, and in the Tulsa plant of the Texas Company.

Q. Do you know whether that Tulsa plant is the one designated in the interrogatories as the No. 1 or No. 2 Holmes-Manley?

A. No, I don't know which it is.

Q. When did you see that one in operation?

A. That was some time last spring.

Q. Can you use a dirtier stock in the Cross process than you use in the Burton?

A. Yes, you can use somewhat dirtier stock in the Cross than in the Burton.

Q. You are not limited, then, to the stocks that you think Burton disclosed?

A. Not absolutely, no. You get a good deal of the same value when you do use Burton's stock, but that does not say you cannot go a little bit further with the present day im-

provements in cracking processes than Burton was able to go with his type of still.

Q. It is not essential, in other words, to use Burton's cracking stock in the Cross process?

A. No, it is not essential.

Q. In the tube and tank process can you use a dirtier stock than in the Burton process?

A. Somewhat dirtier, within the range disclosed by Chamberlain.

Q. But you are not limited to what you think Burton disclosed?

A. Not absolutely limited, no.

Q. Is the Adams process limited to the use of a clean stock?

A. What do you mean by the Adams process?

Q. The process disclosed in his patents.

A. Well, there are many different processes disclosed in his patents.

Q. Would you use any one of them with dirty stock?

A. You could undoubtedly operate them. They would be operable, certainly.

[fol. 1085] Q. To the same extent that Dewar & Redwood would be operable with dirty stocks?

A. Well, you will have to tell me which particular process you are referring to.

Q. Take the process in Adams patent No. 1,320,354, Exhibit No. 185.

A. Of course, that is an apparatus patent, you understand.

Q. You do not think it discloses a process?

A. Oh, it discloses a process, yes. Now the question is could Adams——

(At this point the question was read by the stenographer).

The Witness: They are so utterly different in kind that it is really hard to say whether they could be operated to the same extent or not. I don't know. They could both be operated and make gasoline with dirty stocks, and they would both have to shut down a lot quicker than if they had clean stocks.

Q. You think that the process and apparatus disclosed in this Adams patent could be used commercially without using the gas oil stock that you think Burton disclosed?

A. This "commercially" proposition—I had not said that Adams' process, just as disclosed, would be a commercial process. Of course, the operating costs of electric heating are rather high.

The Master: What do you mean by "commercial process"?

Mr. Rauber: That is a term that the defendants have been using, a commercial process, here.

The Witness: Well, I have used it with regard to the commercial processes I have been talking about.

Q. How is that?

A. I have used that term when I was speaking about commercial processes. There are such processes——

The Master: That is, processes which are being actually used in the industry?

The Witness: Yes.

Q. You do not consider this Adams process as a commercial one, then?

A. I would want to make some changes before I would expect it to make a profit.

Q. Radical changes?

A. No, not radical changes. I would use a great many of the same valuable principles that Adams disclosed, and of course it is entirely operable, but that is not the kind of plant [fol. 1086] I would build today to make the most money. I would make modifications.

The Master: Do you believe that an electrical heater could be operated at a profit today?

The Witness: Well, Adams' disclosure of his electrical heater, you will recall, includes a pre-heating coil in some form, to minimize the amount of electrical energy that you need to put in at that point. That is, you supply part of the heat by direct fire and the rest of the heat by electrical means. And with that modification, doing practically all the heating by direct fire and only the last and most difficult heating by electricity, I think you could undoubtedly operate at a profit with a properly designed apparatus. You would have to add some of the skill of the art to the patent drawings, of course, to make a commercial process.

Q. Without the use of any other invention?

A. You could make a pretty satisfactory cracking process by utilizing only the inventions which Adams disclosed, plus the expedients known in the art.

Q. What are those expedients that you would use?

A. Why, they would have to do with the relative proportioning of the different parts, with the design of the preheater, the design of the electrical heating elements—well, all those things which always come in, in going from the patent drawings to working drawings to build the apparatus, where you figure out exactly the strength of all the different materials that you need in it and design the apparatus accordingly.

Q. Turning to the Adams patent No. 1,327,623, which is Exhibit No. 188, do you think that is a commercial process?

A. It certainly is not used commercially, if that is what you mean.

Q. Could it be used commercially?

A. It would operate commercially, yes—on a commercial scale.

Q. Without using clean stock?

A. Well, you would have to shut down pretty frequently if you did not use clean stock.

Q. Have you any idea how frequently?

A. That would largely depend on how much cracking you were trying to do and just what proportions you put into the apparatus when you came to make the working drawings.

Q. Assuming that you were cracking for gasoline and using the stocks that Adams describes.

[fol. 1087] A. Where? That he describes where?

Q. Various crude petroleums having asphalt and tar bases. That is on page 2017 of the book of exhibits, lines 22 to 24.

A. Well, you would not be able to run a very long time with those—not a long time to make a commercial yield, in my opinion, unless you made radical changes in the apparatus. But you could operate it and you could make gasoline with it.

Q. Could you operate it as well as you do the Dewar & Redwood process?

A. Well, the Dewar & Redwood process is a kerosene-making process. Do you mean as well as you could the Dewar & Redwood.

Q. Dewar & Redwood also made gasoline, did they not?

A. A small incidental amount, yes.

Q. How much?

A. They do not say definitely. It was clear they were mainly after the kerosene.

Q. They say kerosene and mineral spirit, that you could use for carburetting coal gas.

A. Yes, that is a by-product, apparently, of their invention.

Q. They do make considerable amounts of it, apparently, do they not?

A. There is no indication that they do. If they had the proper correlation of temperature and pressure to make kerosene, they would make very little gasoline.

Q. Is that proper correlation necessary in operating the Adams process?

A. Not by any means as necessary, because Adams has additional possible means of producing the cracking in his apparatus besides that which takes place in the main body of oil.

Q. What is that?

A. That is the additional cracking I mentioned, in the superheated film immediately in contact with the very hot surface. You see, Adams is not a black bottom process like some of those we have been discussing, but he has intense localized heating.

Q. Would you class it as a bulk pressure distillation?

A. Yes.

Q. And that is in the same class that Dewar & Redwood is in?

A. Yes. That does not say there are not differences in processes in the same class.

[fol. 1088] Q. It is a liquid phase process?

A. He has a large body of liquid oil present. I do not say he does every bit of his cracking in the liquid phase by any means.

Q. But his pressure must be great enough to hold it in the liquid phase?

A. To hold some of the oil in the liquid phase, yes.

Q. Then the proper correlation of pressure and temperature is not necessary for every bulk pressure distillation?

A. Not for a process where you do not attempt to maintain black bottom conditions—in other words, where you have very hot localized heating surfaces. Another example

is the additional cracking coil that Pielsticker uses beyond his still. The proper cor-elation of the temperature and pressure limits the products which he can make in coming off from his still, but he gets another crack at the vapors by putting them through a highly heated tube, and the same is true of Adams.

Q. Is the proper correlation of temperature and pressure necessary for the Burton tube still?

A. Yes, it certainly is. That is a black bottom, or a black tube process.

Q. For the Holmes-Manley?

A. Yes.

Q. I did not quite understand why it is not necessary for Pielsticker.

A. I was speaking particularly of that modification of Pielsticker, where he takes the vapors which come off from his pressure still and puts them through an additional cracking coil.

Q. Yes, but the other part, if he makes gasoline in that it is necessary there, too?

A. Yes, it is necessary there if he has no additional means of getting the cracking.

Q. Then you think this Adams process is nearer to that in the case of Pielsticker, where he heats his vapors after the pressure has been released?

A. No, I don't think that. It has one feature in common, but that does not make it any nearer.

Q. But so far as the heating around that tube is concerned, they are essentially the same?

A. Oh, no.

Q. What is the difference?

A. Why, Adams heats his flowing stream of oil on the [fol. 1089] way to the bulk supply by intense local heat; Pielsticker overheats and cracks the vapors that have left the bulk supply.

Q. Doesn't Adams do some cracking in this bulk supply?

A. Certainly.

Q. Wouldn't he do most of it there?

A. That would depend on the particular correlation of temperature and pressure that he employed.

Q. He would have to have some sort of correlation, would he not, to do the right kind of cracking?

A. He will inevitably crack the heavier materials in the body of oil. But lighter materials, such as kerosene, if you did not have the correlation, would nevertheless be cracked to a considerable extent by merely passing by the heating surface, just as the white oil which we had here showed definite evidences of cracking in contact with the wire, through the main body of oil at that time was not cracking.

Q. To get any cracking in the main bulk of the oil you would have to have a correlation between temperature and pressure; is that right?

A. You would have to have some correlation. You would have to have a cracking temperature in your body of oil and a pressure adequate to hold it there, but there is no particular correlation you would have to have. The composition of your stock would change. The difference which I apparently have not been able to make clear to you is that in a process like the Burton if we do not crack a particular product, why, we lose all chance at it. In other words, if we do not crack it in the bulk supply, it gets away, so therefore we must hold everything which we want to crack in the bulk supply. In a process like Adams we have a second chance to accomplish our cracking, and even if some of the lighter constituents are not held in the bulk supply, we still can crack them, particularly if they are put back through, as Adams indicates. So that correlation of temperature and pressure is by no means the vital thing in Adams that it is in Burton.

Q. Isn't that also true of the Holmes-Manley?

A. No, it is not. You do not have any of the local overheating there which gives you this highly localized cracking.

Q. Not in this heating coil?

A. No. You could crack to a slight extent, of course, in your heating coil, but you do not get the desired cracking unless you have the correlation of temperature and pres- [fol. 1090] sure, and you ordinarily run the coil at temperatures only very slightly above those in the stills.

Q. How much above the still temperature would you run this Adams process here through the tube?

A. Well, of course, as I pointed out, in Adams there is a superheated film in immediate contact with the heating sur-

face that is far beyond any temperatures in the main body of oil.

Q. Isn't that true of the heating coil in the Holmes-Manley?

A. No.

Q. Why not?

A. It is only a difference of forty or fifty degrees there.

Q. Between the fire box and the heating coil?

A. Not the fire box, but between the metal temperature, and the oil in contact therewith.

Q. How much difference would there be between Adams and that process?

A. There is obviously a difference of several hundred degrees.

Q. That difference is determined by the transfer of heat, isn't it?

A. No.

Q. You have a difference of temperature or gradient between your oil and the tube walls, and when you do you get a certain heat transfer; is that right?

A. No, that is not the whole story by any means. Do you want the whole story?

Q. Yes, let us get the whole story.

A. The differences in temperature depend on the rate of heat input and in the character of the oil and the rate at which the oil is flowing past the heating surface.

Q. If you have a certain temperature gradient between your still walls and the oil you have a certain rate of heat transfer?

A. Not necessarily. It will vary with the velocity of the oil. You might have ten times the heat transfer with the same gradient if your velocity were higher or your pressure were higher, keeping down the size of the vapor film.

Q. That is a matter of velocity, isn't it? The higher the velocity the less temperature gradient you need?

A. The higher the velocity?

Q. For a given transfer of heat.

[fol. 1091] A. For a given transfer of heat from a given area, the less temperature gradient you require.

Q. Then in the Adams process you have a higher temperature gradient because your velocity is less?

A. No, because your heating surface is more limited. Your heating surface here is very, very much smaller in comparison with the bulk supply than it is in the Holmes-Manley, where we have a great big pre-heater. By having a big pre-heater we can put in the same amount of heat with very much less gradient.

Q. Per unit of heat transferring surface, the temperature gradient is directly proportional to the heat put into the oil for the same velocity; is that not right?

A. No, you would have to specify the kind of oil and the pressure as well.

Q. Do they have much of an influence on the transfer?

A. In a case like this, where you have an insulating vapor film, under certain conditions they have a very considerable influence.

Q. If you had a sufficiently rapid velocity you would tear that film away, would you not?

A. Yes.

Q. So that in the Adams process you do not have a sufficient velocity to tear away that film?

A. Well, of course Adams does not say that he has a vapor film there. I think that is probably what he has in most of his operation.

Q. And he probably, then, has too low a velocity to tear it away?

A. Yes. Of course, the velocity required to tear it away might be stupendous, but I presume there is a velocity which would tear it away.

Q. Would it be greater in the case of Adams than it would be in the case of the Holmes-Manley?

A. Well, the Holmes-Manley has no problem of tearing away the film. The Holmes-Manley heating surfaces, being so much larger, have so much smaller gradient, that the two cases are not strictly comparable in the velocity required to prevent the formation of a vapor film.

Q. Of course, if there was no velocity at all there would be some film formed?

A. In the Holmes-Manley?

Q. Yes.

[fol. 1092] A. I presume so.

Q. Is there any correlation of temperature and pressure necessary in the Cross process?

A. No, the correlation of temperature and pressure has a very minor influence on the quality of the distillate. I do not mean that correlation may not be required for other purposes, but——

Q. (Interposing.) It is not *that* the correlation that is described in Burton?

A. No. When you have a process where you subject both the vapor and the liquid to cracking temperatures, then the importance of the correlation practically drops out.

Q. And in Cross you have both vapor and liquid subjected to cracking?

A. Yes.

Q. That vapor is vapor of the material sent through the coil, of course; it is not permanent gases?

A. Well, it is to a considerable extent permanent gases, and the light ends of gasoline. I don't know just what its composition is.

Q. A mixture of both, is it?

A. I should certainly assume so. I have never analyzed it.

Q. Is it your opinion that those skilled in the art understood from the Burton patent that it was necessary to use gas oil?

A. I would not say that Burton said it was necessary to use gas oil. They would certainly understand that was what Burton disclosed and what Burton said worked.

Q. And that that was an essential feature of the Burton process?

A. No, I do not think that he particularly emphasized that that was the only stock that would work.

Q. You feel that a person skilled in the art, reading that patent in 1913, would not understand from it that it was limited to gas oil?

A. What do you mean by limited?

Q. Well, that you had to use gas oil or a similar distillate boiling around five hundred degrees?

A. Well, certainly the patent did not say—I do not quite get what you are driving at. The patent did not say you can use its process on only one stock. What it did clearly say was that its process will work on one particular stock.

Q. And that was one of the points of novelty over the prior art?

[fol. 1093] A. Well, it mentions it in the claims, if that has anything to do with it.

Q. You think that one reading the claims would understand that gas oil was the oil intended?

A. Reading the claims in the light of the specification, certainly.

Q. At that time you were not one of those skilled in the art, I suppose?

A. No.

Q. Where do you get this idea as to what a person skilled in the art would understand?

A. From my general knowledge of the cracking art and the refining art.

Q. From things you learned since you went with the Standard Oil Company of Indiana?

A. Partly. Partly things I learned since and partly things I learned before I went with the Standard Oil Company of Indiana.

Q. Did you get it from any books?

A. Yes, in part.

Q. Reading from page 1929 of the exhibits, being the first page of the Burton patent No. 1,049,667, Exhibit 176, I find the following, line 59:

"The object of my invention is to provide a method of treating the aforesaid residue of the paraffin group of petroleum by distillation and condensation of the vapors thereof, whereby the resultant product of low boiling points shall be of the same paraffin series and thus free from the objection mentioned; or, in other words, whereby conversion of the petroleum of that series into products belonging to the ethylene series shall be avoided."

Do you understand that the product produced in the Burton process is just the same as that of natural petroleum and that it avoids producing hydrocarbons of the ethylene series?

A. What do you mean, "just the same as natural petroleum?" You mean just the same as gasoline or the petroleum itself?

Q. That is the gasoline produced by his process is the same in regards to its ethylene content?

A. No, it is not the same. It is largely saturated in composition, which is the thing that Burton is emphasizing; not entirely, but much more largely so than the prior processes that he was considering.

[fol. 1094] Q. How would the products produced by Burton in this respect compare with those produced by Dewar & Redwood?

A. Well, Burton's product was gasoline and Dewar & Redwood's was mainly kerosene. That is the outstanding difference between the two.

Q. And some gasoline?

A. And some gasoline, yes.

Q. How wou-d the gasolines from the two processes, that is Burton and Dewar & Redwood, compare in respect to ethylene content?

A. I should expect Dewar & Redwood's to be somewhat higher. That is, I do not mean to indicate as high as those which you would get from a high temperature tube-cracking process for example.

Q. Why would they be higher?

A. Because they are made from heavier boiling materials which contain a less proportion of hydrogen to carbon. In other words, in cracking a residuum stock you do not get as good quality gasoline as you would get from cracking a distillate stock, which has a higher ratio of hydrogen to carbon.

Q. Is that because the Dewar & Redwood process is operated at a higher temperature than the Burton?

A. No, that is because of the character of the stock.

Q. On page 1895 of the exhibits, being part of "Exhibit No. 169," the file wrapper of the Burton patent, I read the following, which is in the fourth paragraph:

"The Dewar patent discloses a process in which pressure is produced in the still in addition to the pressure produced by the vapor coming off as distillate. The result is that this raises the pressure in the still unduly and because of this undue pressure, the temperature of the liquid being distilled would be raised so high in proportion to the pressure as to prevent the distilling off gasolene in the paraffin series, but would result in distilling off gasolene of the olefines series";

is that true?

A. I do not agree with the statement there, no. Excuse me, I do not mean to say that the temperature may not, in fact, the temperature probably is, somewhat higher in the Dewar & Redwood process, being a kerosene making process and working with heavy stocks and that may have something to do with the difference. But the difference which I am speaking of, and I think the largest difference, is in the character of the stock.

Q. Are you sure that it would require a higher temperature to produce kerosene from a heavier stock in the Dewar & Redwood?

A. It would require a higher temperature for a given pressure, yes, than to make gasoline say from a lighter stock.

Q. Simply to push the kerosene out of the still?

A. Yes.

Q. But in that case you would not heat the liquid to so much higher temperature as to give a larger proportion of unsaturated; that would not compare with a flash cracking operation.

A. It would not compare with the flash cracking process, no. It has just a little trend in that direction. That is all there would be to it.

Q. In your direct testimony, I believe you said that the only patent formally cited was the Testelin & Renard German patent?

A. Yes.

Q. You were under the impression at that time that that was a vapor phase flash type of process?

A. No, it is a tube cracking process.

Q. Without vaporization?

A. Oh, vaporization, yes.

Q. You know there are two Testelin & Renard patents?

A. I do not know whether I have seen them both or not. I am familiar with the one in this case.

Q. Have you a copy of it?

[fol. 1095] A. It is in the record.

Q. I do not believe so, not the German.

A. I say, I am familiar with the one in this case. I mean in the Government's case.

Q. There are two?

A. In this case?

Q. Yes.

A. Well, then, it is in the record.

Q. We will get them. Exhibit 147 is one.

A. Maybe you are thinking of Renard as the other one.

Q. 141 is Testelin & Renard.

A. I think there is only one in this case by Testelin & Renard.

Q. You may be right; I do not find the other one. Apparently it is not the same because he says here on page 1896 that Testelin proposes to keep the pressure on his superheating coil to prevent the boiling of the liquid. That is not the condition you had in mind in the Testelin that you were speaking of, is it?

A. I said nothing about whether he had liquid or vapor, as I recall. I said it was a tube type of cracking process.

Q. If he kept it at a pressure sufficiently high to prevent the boiling of the liquid, it would not be superheated to form ethylene products, would it?

A. I do not think that description on page 1895 corresponds to the description actually in the Testelin & Renard patent.

Q. It is a different type of process, I think. I thought both of the patents were in the record.

Now, in the affidavit filed in this Humphreys patent, he mentions a test——

Mr. Wiles: You mean Burton?

Mr. Rauber: Yes.

Q. The Humphreys affidavit in this file wrapper, on page 1897, he describes a test in which he superheated vapors to crack them?

A. Yes.

Q. Is that at all like the Dewar & Redwood?

A. No. As I pointed out, he was apparently referring to Testelin & Renard, which was the type of process which did use that type of stock.

Q. But if Testelin & Renard had prevented the liquid [fol. 1096] from vaporizing, then of course it would not refer to Testelin & Renard, either, would it?

A. Well, that assumption, of course, does violence to the facts. You are correct on that assumption, however.

Q. Then this would not apply to the Testelin & Renard?

A. If Testelin & Renard did not vaporize then that would not be the Testelin & Renard process.

Q. Would you get the same sort of results in this superheating of the vapors as you would in Dewar & Redwood as to ethylene content?

A. Of course Dewar & Redwood had an additional reason for producing more olefins in that their entire long structure was in the furnace and exposed to fire, both above and below, and therefore they would get a certain cracking of the vapors because the top of the retort above the liquid oil would probably be hotter than the bottom of the retort which was, to some extent, kept cool by liquid.

Q. The vapors pass through there very rapidly, do they not?

A. Yes, you can pass vapors through there very rapidly, and yet get cracking, if you have a high enough temperature.

Q. In your opinion would you get just the same sort of results in Dewar & Redwood as you do in this superheating of the vapors, same results as to——

A. No, you would not get the same results, but you would get a larger proportion of olefins in a patent like Dewar & Redwood than in a patent like Burton.

Q. How much larger?

A. I don't know.

Q. Very slight?

A. Well, I would have to know about the extent to which the top was superheated. You would have to build one just about like that and see just the extent to which the flames might play around, and of course, the particular kind of refractory you used would have a considerable bearing on how much cracking you got there.

Q. Did you ever try to cut the Dewar & Redwood process?

A. Yes, as near as I could. That is, I was not able to get the same type of tubular refraction they showed, so I made it brickwork.

Q. It was not quite the same then?

A. Not quite the same, no.

Q. Wasn't this contention of Burton's that he got less ethylene products, wasn't that understood to be his contribution to the cracking art?

A. Understood by whom?

Q. Those skilled in the art?

A. No, I do not think so. It could be so misinterpreted by a hasty reading, but those skilled in the art did more than read it hastily.

Q. As I recall from your direct testimony, you stated that Bacon, Brooks and Clark in their patent did not advocate the use of gas oil or clean stock because they did not have the ben-fit of Burton's disclosure, is that correct?

A. I don't think that that was what I said. I think my statement was they had not taken advantage of Burton's disclosure, but I am not sure.

Q. Apparently they did not get that out of Burton's patent, is that right?

A. If they read Burton's patent. They may, however, have felt it was more desirable to get another process which would crack dirty stocks because they were so much cheaper.

Q. In a book by Bacon and Hamor, Volume 2, published in 1916, page 566, I find this statement:

"The claim is made by Burton that by replacing the pressure control valve beyond the condenser so as to condense the volatile gasoline vapors, produced by distilling heavy petroleum oils under about seventy-five pounds pressure, olefins are not produced."

Is this the same man who testified about that in the Dubbs case?

[fol. 1097] A. Yes, Bacon, who is one of the authors of that book, some eight years or ten years later testified at Kansas City.

Q. He would be one of those skilled in the art, would he not?

A. I think it is a question whether you could call him at the time he wrote the book, one skilled in the art. He is now, because he has gone into the industry much more thoroughly. His particular type of knowledge then was that of an outsider looking in and there were many things that were known in the industry which might not be known to him.

On the other hand, in many ways, he was certainly much more familiar with the literature of the industry than many of those in the industry.

Q. He was quite capable of reading and understanding a patent, I suppose, was he not?

A. Well, that depends on whether that patent required an intimate knowledge of refinery processes or simply a knowledge that could be obtained by one outside of the industry.

Q. You do not think a person having sufficient knowledge to write a book of this type could understand that from this patent?

The Witness: Of course, we are talking about this book as if — were a book written by one author, whereas it was a book written by two, and this part may have been written by somebody else and Bacon did not look into it particularly; so I should not want to pin that on Bacon personally, not knowing any more than I do about it.

Q. This is a standard book in the art, is it not—Bacon and Hamor?

A. It is a very well known book, and much referred to, but it consists, largely, though not entirely, of compilations of material from the literature, as you will note. Practically every page is full of reference to prior articles and patents.

Q. I am going to call your attention to page 428 of the Hand Book of the Petroleum Industry, by David T. Day in the lower paragraphs—can you find anything on that page that shows that Day appreciated, from the Burton patent, that it was necessary to use a gas oil?

A. He discusses the process only very briefly, and states that the principal difference between Dewar & Redwood, and Burton, was in the emphasis that Burton placed upon condensation under pressure. He doesn't say anything about any other——

The Master: I might say that I have a distinct impression that Burton says his charging stock is "this residue," and this residue is fuel oil and gas oil, with boiling points ranging from about 600 degrees to 700 degrees Fahrenheit.

The Witness: Of course, I have already pointed out, in my direct, that Burton attributed all of his gain to the factor which was responsible for only part of his improvement in quality.

The Master: What the defendants have to maintain here, is that Burton was the first man that pointed out that this residue, treated in the manner in which Burton specified in his patent, was novel, and was useful.

Mr. Rauber: And an invention.

The Master: Well, if it was novel and useful it was invention.

Q. In your direct testimony I understood you to intend to say that the Riebeck process is not a commercial process—is that correct?

A. No, I said that the Riebeck process could not be operated on a commercial scale, to produce marketable gasoline. I did not say it was not commercial, or operable. You can make small amounts of gasoline, and if you have enough of a margin in price between the cheap residue and the kerosene, and it would be profitable.

Q. It had about the same relation to the size of the stills of that time, that the Burton still has to the present crude stills—about one quarter of the size—is that right?

[fol. 1098] A. I don't know how large the stills were in that day. My impression is that they were quite large. I don't know whether they were in Riebeck's particular factory, or not. It may have been that the amount of coke produced by their stocks was so large that they had to use larger stills than we commonly had.

Q. The Burton stills are about one-quarter the size of the crude stills, aren't they?

A. Do you mean the capacity that they hold? Yes, the Burton stills are about a quarter to a fifth the size—now, wait—yes, in their volume or capacity—not in their lineal dimensions.

The Master: Are you now referring to the crude stills that distill at atmospheric pressure?

The Witness: Yes, sir.

Q. You calculated that if the Riebeck, or Krey cracking stills were expanded to hold, or to handle the same quantity as the Burton still, they would require walls over a foot thick, did you not?

A. To have the same ability to handle coke, yes, sir.

Q. I understand you changed that one foot to eight inches—is that right?

A. No, that was another calculation. I am not sure what I testified about the Riebeck still. Have you my testimony there?

Q. Yes, here it is.

A. They were a lit-le over an inch and a half thick. Let's see. No, that is too high. There was an error there. That

was an error in arithmetic, apparently. They would be around seevn inches.

Mr. Wiles: Does that refer merely to the strength point?

Mr. Rauber: Here is his testimony.

Mr. Wiles: Let me see it.

The Master: That is where he was testifying with relation to hot bottoms.

Mr. Rauber: Yes, sir.

Q. Is that in direct proportion to the diameter of the stills?

A. Yes. I was figuring on a basis of direct proportionality.

Q. Riebeck had cast iron stills, didn't he?

A. Yes, sir.

[fol. 1099] Q. And the tensile strength of steel is much greater than it is for cast iron, isn't it?

A. At what temperature?

Q. At ordinary temperature.

A. Yes, it is very much greater, at ordinary temperature.

Q. And isn't it quite a lot greater at 700 degrees Fahrenheit?

A. I really don't know. I haven't seen the cast iron curve for the higher temperatures.

Q. Would you expect it to be?

A. I would expect steel at 700°, to be still somewhat higher. Of course we are more interested in the red heats than we are in the strength at 700. It is a question of the strength when they get a hot bottom.

Q. How would the strength of cast iron compare with that of steel, at red heat?

A. I really don't know. I have not seen the curve of cast iron at high temperatures.

Q. As a usual thing, cast iron has less strength than steel, doesn't it?

A. At ordinary temperatures, it does, undoubtedly.

Q. In this Krey process they used flat bottoms, didn't they?

A. What part of the Krey process are you speaking of, now?—what exhibit?

Q. "Exhibit No. 246"?

A. I have not been able to find anything which says definitely whether it was flat bottom still, or an ordinary cylindrical still.

Q. On page 4226 of the Exhibit Book, about the middle of the page, he speaks of the cover of the still.

A. Yes.

Q. That would indicate a cheese box type, would it not?

A. No.

Q. You would hardly have a cover on a cylindrical still, woul- you?

A. We have a manhole cover on every still.

Q. But this cover carries a safety valve and filling pipe—you wouldn't put those on a manhole, would you?

A. The safety valve is often on a manhole.

Q. And a filling pipe?

A. Not usually. I see no great difficulty in a small still, in putting it on there. In a large still you would want to be [fol. 1100] able to take the manhole cover off without disturbing your filling pipe.

Q. Cheese box stills were commonly used at that time, were they not?

A. I believe they were.

Q. And, of course, if you had a flat bottom you would have a much thicker plate than if you had a cylindrical still—isn't that true?

A. It certainly would be unreasonable for him to emphasize a concave cover, where not exposed to the fire, and then have a flat bottom where it was exposed to the fire. I don't think he could possibly have had a flat bottom still, in view of that.

Q. But that was the common type of cracking still, wasn't it?

A. Cracking without pressure?

Q. Yes.

A. Well, yes, sure—that was quite a different art.

The Master: He says his still can sustain a pressure up to ten atmospheres.

Q. In 1912, of course much better stills were available than were available in 1885—isn't that true?

A. I presume so.

Q. And you would naturally expect to be able to reduce that thickness of the wall, would you not?

A. Yes, you could probably get equal strength at a red heat, with some less wall thickness, in view of the improvements in steel.

Q. And if you used the same factor of safety as Riebeck used, you know that you could use plates of about half an inch in thickness, couldn't you?

A. Oh, no.

Q. Have you calculated that?

A. Half an inch, to compare with Riebeck's inch and a half thickness, on a small still?

Q. But he was using cast iron.

A. Yes, but there is no such difference as that in the strength of the materials.

Q. Oh, no, but you can calculate the thickness of materials required for a pressure of ten atmospheres, couldn't you?

A. With a red hot bottom, or not?

Q. With a red hot bottom,—well, he specifies 400 or 450 degrees Centigrade.

[fol. 1101] A. That is not what we need the strength for. We need the strength for the hot bottoms, and that is all I am talking about, and a half inch plate would certainly nowhere near stand a hot bottom on a commercial sized pressure still. We know that to our sorrow.

Q. D- you find any statement in Riebeck that he has a hot bottom?

A. Yes, sir. Of course, the fact that he split the stills is pretty good indirect evidence, and in one of his references he certainly speaks about red hot bottoms.

Q. He states that that still was replaced by the manufacturer, free of charge—did you notice that?

A. That was one of the two that burst, I believe.

Q. And that the other was not intentionally treated carefully—or was, intentionally not treated carefully?

A. Yes.

Q. Simply to test out whether it could be broken—isn't that right?

A. I don't know what the object would be of not treating carefully. At least one such reference to heating the bottoms red hot is in the Riebeck patent, itself, on page 1290, of the Book of Exhibits—the middle of the first full paragraph—where it says, "When the parts swept by the fire are heated to a red glow," and I think there is another reference in one of his publications, to the same thing. But I presume that is enough to cover the point.

Q. What do you consider to be the essential characteristic of Humphrey's patent, No. 1119700?

A. The essential features of the Humphreys patent are in the partial cooling of all the vapors and the condensing of substantially everything but the desired product, and returning that by a long pipe countercurrent to the vapors, so as to strip out of it the lighter constituents, and to warm it up before it reaches the still.

Q. Dewar & Redwood cool all their vapors coming from the still, don't they?

A. Not in the sense that Humphrey does.

Q. If you cool the vapors you simply cool them, and that is all there is to it.

A. No, you can cool part of them to a low temperature and let the others go through at a relatively higher temperature. It is a question of cooling, and the action that you get when you pass the vapors through the center of a cylinder whose axis is perpendicular to its axis, is quite [fol. 1102] different from what you get when you pass them through an elongated passageway such as Humphreys'.

Q. Dewar & Redwood don't say that they pass through such a cylinder, do they?

A. No, but I am talking about the apparatus as described.

Q. They simply state that they wanted to cool the vapors, don't they—isn't that right—the vapors coming from the still?

A. I don't think they made just that statement, but they clearly disclose that their means of cooling is this drum, and they don't say cooling all the vapors, which is an essential feature of Humphreys.

The Master: What Dewar & Redwood do you refer to?

Mr. Rauber: "Exhibit No. 103."

The Witness: I don't see where they make any disclosure of cooling the vapors, in any such words. They speak about "If you desire to operate by the cracking methods above referred to." Then they also say that the—I see nothing else but that.

Q. I call your attention to lines 55 to 60 of page 1360 of the exhibits.

A. 55 to 60?

Q. Yes.

A. There is nothing there about cooling.

Q. It says, "During the distillation such of the vapor as may be condensed in the stillhead, C-2, or such liquid as may prime up into the stillhead flows back into the body of liquid in the retort."

A. Yes, but there is nothing there about cooling, however.

Q. The only way you could condense them would be to cool them, wouldn't it?

A. Well, in that type of apparatus, yes.

Q. If you condense the vapors it is the heavier ones which condense out, isn't it?

A. That depends a good deal on how your condenser works. You can have certain kinds of a condenser which condenses both light and heavy, on a bypass and others which condense just the heavy and make a fairly good fractionation.

Q. But if you cool the vapors and let those that are uncondensed flow through in a properly designed reflux, it will be the lighter ones that flow through, will it not?

[fol. 1103] A. That is the whole purpose of a properly designed reflux. Dewar & Redwood——

Q. It is a properly designed reflux, is it not?

A. (continued). Dewar & Redwood had a thing which would accomplish their purpose; that is, mainly to stop entrainment, and to return any such material as might that happen to condense. But that is entirely different from Humphreys, who deliberately discloses a certain kind of condensation, which is to partly cool all of the vapors and let only the desired product escape, and then return the condensate through a long stripping section. Dewar & Redwood had nothing resembling that.

Q. The only purpose in condensing at all would be to condense the heavier ones, would it not, in Dewar & Redwood?

A. I do not see very clearly that they had a purpose in condensing. As I pointed out, they seemed to think that was a lot of bunk.

Q. They clearly made provision for condensing some of the vapors?

A. No, they said—what line was that, 55?

Q. 55 to 60.

A. They make no specific provision for condensing, but they made provision for returning such as might condense, which is quite a different thing.

Q. That which would condense would be simply because of cooling, which would mostly amount to unavoidable condensation, you might call it?

A. Yes.

Q. And the heavier portions all condense first at the higher temperature?

A. Not in a device of this kind, that is far from being true. In a device of this kind where you have dead ends—

Q. Just a minute. Your impression is that the vapor goes right straight up through the center?

A. No, starts up through the center and goes off a little to one side. That still leaves a certain amount of dead ends in the right——

Q. It is a matter of properly constructing the reflux, then?

A. For what purpose?

Q. To return condensed vapors?

A. No, this will return the condensed vapors all right. I have no objection to its returning action. It won't strip them, of course, but it will return them. If you want to [fol. 1104] design it properly to condense a large amount of vapors and to cool all of the vapors you would have to design it quite differently from this.

Q. It would be a different design of the same still, the same way that you would modify the Adams to make it commercial?

A. No, because in modifying the Adams to make it commercial, I would still be trying to do what Adams tells me to do, and if you made such a modification as this on Dewar & Redwood you would be trying to accomplish a different thing, which Dewar & Redwood were not thinking about.

Q. In condensing to return the condensate; they distinctly mention that, do they not?

A. No, they mention returning what may condense.

Q. Which applies to what may condense?

A. Yes.

Q. And it is the heavier portions which condense first?

A. Not in this type of apparatus. The condensate which

they would get in a dead end, such as they show, would be both light and heavy materials.

Q. In your direct examination, I understood you to say that the important thing in Humphreys was to have the reflux surface greater than the heating surface of the still, is that right?

A. No, I said that that is the way it worked out, but the important thing is just exactly what I have stated with regard to Humphreys, regulating the proportions so as to get cooling off of all of the vapors, allowing only the desired product to escape and returning the other materials counter-current to the hot vapors and to heat them and ddrive off the light ends. These are the essentials of Humphreys.

Q. And that would be accomplished in a fractionating tower, would it not?

A. No, not in any fractionating tower.

Q. In what type would it be accomplished?

A. Entirely dependent on how you design the tower and how much cooling you supply, and many other things. It could be accomplished in a properly designated tower.

Q. In a fractionating tower you condense the heavier portions and strip out the lighter, is that not true?

A. Well, now, are you talking about——

Q. A bubble tower.

A. Say a bubble tower where the vapors go in at the bottom and pass out at the top and the liquid runs down [fol. 1105] through the tower and back into the still; is that the kind of bubble tower you are talking about?

Q. Yes.

A. In that kind of a bubble tower, if you had adequate reflux by some suitable cooling means, and if you had enough plates in the tower, you could accomplish Humphreys purpose.

Q. That is, to return the heavier and leave the lighter go through?

A. Yes.

Q. And a properly designed bubble tower would do that, would it not?

A. Well, the whole emphasis is on the properly designed. "Properly designed" would rule out, of course, many bubble towers.

Q. You mean those that are too small or too large?

A. Too small or to large or do not make adequate provision for returning the liquid or that would coke up very quickly in a device of this type. There are a lot of things. Designing a bubble tower for a pressure still is quite a tricky problem.

Q. Referring to the Pielsticker patent, Exhibit No. 133 and Exhibit 134?

A. Yes.

Q. There is an element there marked W.

A. Column still W.

Q. Column still W. That is the same as a bubble tower, is it not?

A. Not as I see it, no. I see no indication of that being a bubble tower. It says it is filled with baffle plates and the only part that it shows is filled with baffle plates.

Q. Where are you reading from?

A. I am not reading, I am simply——

Q. On page 1671, line 100, it says, "That the products of distillation may be fractionally separated into a number of oils——"

A. Excuse me. What page was that again, 1671?

Q. 1671.

A. Line?

Q. 100.

A. No, I do not——

Q. 99, I mean. "May be fractionally separated into a number of oils of graduated specific gravities by connecting the heating coil with the column still W, which may [fol. 1106] be similar to that used in the distillation of benzol."

A. Yes.

Q. Are not the common column stills used for benzol of the bubble tower type?

A. Yes, but why not read the next clause: "The body of which column still—I also divide by baffle plates."

Q. Yes, "also divide," but it could not be a column still without being a bubble tower, could it?

A. Why, certainly. The term "column stills", as I understand it, applies to any type of fractionating tower.

Q. The idea being that you would let the lighter vapors go through and the heavier ones come down?

A. That would depend on what is disclosed with regard to it.

Q. Isn't that what a column still does?

A. That is ordinarily what a column still does.

Q. And that is what it would do in this case too, is it not? That is the only purpose it could have, is it not?

A. Well, a column still is a very efficient device for removing entrainment, in which case you have practically no condensation. In fact, we very frequently use a few plates in column stills with substantially no liquid coming down; just to knock out entrained drops.

Q. That is the type you use in distilling benzol too, is it?

A. Same type of column.

Q. Only to prevent entrainment?

A. What is that?

Q. I say, is it only to prevent entrainment?

A. I am not saying it is used in the same way as in the Benzol industry, but it is the same type of column.

Q. It does return the heavier and let the lighter go through?

A. It knocks out the entrainment and possibly there is a little additional condensation which takes place along with it, but it does not return any substantial amount of liquid down the tower.

Q. In the benzol industry?

A. Oh, in the benzol industry it, as a rule, returns a considerable amount of liquid, the condensed part.

Q. If you put one of those towers on top of a pressure still it would return the heavier and let the lighter go through, would it not?

[fol. 1107] A. It might or it might not. It depends upon how you operate.

Q. How would you operate so it would not return the heavier and let the lighter go through?

A. By keeping it heavily lagged, with rapid circulation, you get only a little bit of the heavy ends back and let the rest go through.

Q. This patent says nothing about lagging, does it?

A. No, it also says nothing about liquid coming down.

Q. It is the same type as used in the benzol industry?

A. I say the type of the tower and the use to which it is to be put are two different things. You can use the same type of tower for two different things.

Q. Did you ever know of this type of column still being used except to separate out the heavier and let the lighter go through?

A. What do you mean, the benzol fractionating tower, as used in the benzol industry?

Q. For fractionating benzol.

A. I have never known its being used in the benzol industry for just knocking out entrainment, but I am not particularly familiar with the benzol industry.

Q. You would have to change it some to prevent it from operating in the usual way?

A. No, they are ordinarily lagged and you would not have to make any particular change.

Q. If they are lagged they have a cooling coil in the top, do they not?

A. Yes, they often do.

Q. That is the most efficient type?

A. But you do not need to put anything through that cooling coil. The amount of cooling medium they put through there, depends upon how much cooling they want. If they do not want any cooling they would not put anything through.

Q. That is the only purpose of using the benzol fractionating column, is it not?

A. In the benzol industry?

Q. Any place it is used?

A. No, I just made clear a number of times we had used such columns for other purposes in the petroleum industry.

Q. Of the type used in the benzol industry?

A. Essentially the same, yes.

Q. Would you have in the benzol industry a column still [fol. 1108] or a fractionating column rather, lagged and without a cooling coil at the top or any cooling means at the top?

A. Well, the ordinary columns that would be used in that industry would probably have some means for cooling. That might simply consist in pumping back some of the condensate. It frequently did.

Q. It would be perfectly senseless to put that in and not use it, would it not?

A. No, it depends upon what purpose you want it for. The cooling means might simply be a pipe which would enable you to pump water or condensate or anything else back in there to furnish the cooling. Now, there would be nothing particularly foolish in not using it if you did not want that cooling.

Q. Why would you install it if you are not going to use it?

A. Because it is a column in the benzol industry.

Q. To be used in the same way?

A. Well, Pielsticker does not say that. Furthermore, he does not say what purpose he could possibly have in returning any liquid, because it does not go back to the still. It goes out with the tar. It would be degrading his distillate unless it were merely entrained drops that were knocked out.

Q. It would return to the bulk container, would it not?

A. No, it goes out with the tar. There is no way of getting it back to the still. Of course, I have previously discussed the question. I don't believe at all that it is even hitched to the still. I do not believe there is any still in the system when he used the tower. I think it is connected on to the heating coil, as he stated; but if it were hitched to the still, there is no way for liquid to get back from the base of the tower to the still. The liquid goes out through the tar draw-off, W, and the steam ejector Z.

Q. There are much simpler ways of stopping entrainment than to use fractionating columns, aren't there?

A. They are simpler, but much less effective, yes.

Q. And the very purpose of a fractionating column is fractionating, is it not?

A. That is the purpose for which it is ordinarily used.

Q. When you install one, you usually install it for that purpose, do you not?

A. Usually yes; by no means always.

Q. And if you install this column in Pielsticker for its usual purpose and operate it on pressure you would return the heavier parts of the vapors, would you not, and let the lighter parts go through?

[fol. 1109] Do you understand that question?

A. No, I don't know what you mean. Return it to what?

Q. Return it to the bottom of the still in just the way Pielsticker says here.

A. You mean the bottom of the column still?

Q. Yes, that would be the only place you could return it, would it not?

A. What is that?

Q. That would be the only place you could return it to, would it not?

A. If you did use it as a fractionating column and condense, the liquid would run down to the bottom unless it were drawn off at some intermediate points in that tower, which he rather seems to imply might be done.

Q. Where does he imply that?

A. Where he says you will get a number of oils of graduated specific gravities. The only way he could do that is, of course, to draw off at intermediate points in the column.

Q. The heavier vapors would always condense and go to the bottom of the still, would they not; go to the bottom of the column I mean?

A. Not necessarily. Whatever did condense, if any did condense, would be heavier than what came out above. Of course, the still in this sense is not a still as we ordinarily term it. That is perfectly clear. There is no body of oil in the still.

Q. Where does he say there is no body of oil?

A. Why, it is indicated by the location of the inlet and the tar draw-off. He certainly is not going to shoot in his vapors underneath a layer of tar and entrain the whole mass up into the tower.

Q. Are you sure that there are vapors coming out of that pipe?

A. I should understand that there were both vapors and liquid; a mixture of both liquid and vapor.

[fol. 1110] Q. In the Burton tube still the vapors and liquids go in at the bottoms, do they not?

A. Yes.

Q. And in the Holmes-Manley?

A. Yes.

Q. And in the tube and tank, do they not?

A. Yes.

Q. And they enter the bulk still the same way that Pielsticker does, do they not?

A. I should hardly say that at all. They are going into a large body of oil with entirely adequate vapor space to separate vapor from liquid. Here you have a shallow passageway between the baffles. If you tried to keep it filled even up to the bottom baffle plate you would get serious entrainment.

The only thing that they are intending to do is to bring in the vapor and liquid and draw off the tar as fast as it comes in. There is no body of oil there.

Q. It is a continuous process, then?

A. A continuous process?

Q. Yes, drawing off tar continously and putting it through continuously?

A. Drawing off tar continuously and putting it through continuously, yes; just as is done characteristically in the pipe still art.

The Master: The question would be, would it not, if Pielsticker does the same thing, does he do it in the same way as Humphreys?

The Witness: Are you asking that?

The Master: I am just stating that. I am asking if that would not be the question at issue. Is it not conceded that Pielsticker did the same thing, namely, that he returned certain of the condensation of the vapors back into his tar that was going back into his still, the question would be did he do it in the same way that Humphreys did?

Mr. Wiles: This of course, we contend, does not go back into the still at all. This column W is not used with the still. This is a substitute for the still as was clearly shown.

The Master: That is used with the reaction tank?

Mr. Wiles: No.

The Witness: No.

The Master: Isn't 3 the reaction tank in Humphreys?

Mr. Rauber: It is a mere matter of Patent Office drawing. [fol. 1111] In one case he shows in figure 6 a long horizontal still, where he might let his tar drop back. In figure 10 he shows——

The Master: It is not what he might do but what he does do.

The Witness: I went through that in considerable detail, if you recall.

The Master: I understood that at the time.

The Witness: But in any case, the tar never gets back to the still, even if the tower were on the still.

Mr. Rauber: He shows no other place for it to go, but down into the place where the vapors come from, does he?

The Witness: It goes to exactly the same place the tar goes that is drawn from his still. It is the same kind of draw-off and everything.

Q. Drawn off with the tar?

A. What is that?

Q. Drawn off with the tar.

A. No, it goes to the tar tank. The tar is withdrawn from the column still W goes to a tar tank, just the same as tar that is withdrawn from the still when he is using a still.

Q. Through what pipe does it go?

A. It doesn't show—that is, it shows an outlet pipe of course, Y.

Q. Or P and Q, if it is on the tank L in figure 6?

A. Oh, no it couldn't go back into there at all.

Q. If W replaced R?

A. Of course that is a ridiculous assumption. There is not the slightest evidence of that, because there is a pipe coming into this column. It is not set on top of the still, but even if W——

Q. Not necessarily, because the baffles X might have the same relation in that lower part that the baffle N had in the chamber L, is that not true?

A. No, I do not see any——

Mr. Wiles: It is clearly contrary to the disclosure.

A. (continued). I see no indication of any such construction at all. I cannot conceive of any way you could arrange the two that will put the tar, which comes out of the bottom of this column, any place but into the tar tank.

Q. It flows down from this column W down on the plates X, down in the lower part of figure 10?

A. No, I should assume that most of the tar was what came in from the heating coil.

[fol. 1112] Q. No, it comes down to those plates X in figure 10, does it not?

A. It might and it might not. That depends upon how much it refluxes.

Q. There is no other place it can go as shown in the drawings, is there?

A. There is no showing that there is anything coming down at all.

Q. The reflux couldn't come down——

A. It doesn't show there is a reflux.

Q. He says he uses a fractionating column?

A. Yes, he says that.

Q. He must have something to fractionate if he uses a fractionating column?

A. That is not necessarily true. We use fractionating columns to stop entrainment.

Q. Where?

A. At the Standard Oil Company at Whiting, on our stills.

Q. Fractionating column?

A. A fractionating column.

Q. How many plates would it have?

A. Four or five.

Q. That is not the type shown here in figure 10?

A. No, we use bubble plates. Baffle plates are a good method of stopping entrainment, however. They are better for that than they are for fractionating.

Q. You say they have baffle plates in benzol fractionating towers, or columns still, rather?

A. I have not said that. I do not know whether they do or not. Evidently this one has.

Q. In the lower part only?

A. He says, "The body of which I divide by baffles plates X".

Q. Which?

A. "The body of which I divide by baffles X".

Q. Where do you read that?

A. I read that in the——

Q. What line?

A. Line 104 and line 105.

The Master: What page?

Q. "I also divide——"

The Master: What page are you reading from?

Mr. Rauber: Page 1671.

[fol. 1113] A. Surely, "I also divide".

Q. Implying that he also does something else?

A. Presumably.

Q. (continued). That putting in baffle plates is not the only thing that he does?

A. No. He undoubtedly has to do several other things with baffle plates.

Q. And put several other things in?

A. No, I do not think that is implied. You read that along and you will see he is just giving an additional detail that might not be understood from his mere reference to a column still as used in the benzol industry.

Q. That he, in addition to that, also puts it in his patent?

A. I would not get the indication that that was necessary in addition to what was already in it.

Q. Of course, you would not want to admit it. Coming to the next exhibit, the patent to Lang No. 954,575, Exhibit No. 135: In that case he passes his vapors through a long series of cooling means, does he not?

A. Yes, he does.

Q. And in several of those sections the vapors flow down in close contact with the condensed liquid?

A. Well, he does not flow them in any place countercurrent to the condensed liquid to get the stripping action. He has parallel flow, then he keeps tapping it off. He does not have any of Hupmhreys' countercurrent flow and stripping.

Q. Humphreys has not a very efficient stripping arrangement, has he, with the liquid flowing down the lower part of the pipe and the vapors up on top?

A. Well, it is surprisingly good. It is not the best, of course.

Q. No. And Dewar & Redwood might be much better than you expect, too?

A. Well, but Dewar & Redwood have these dead end propositions. That is what defeats them. They also have only a few inches apparently to do any stripping in, that very short neck; whereas Humphreys has two pipes forty feet long. There certainly is a tremendous difference between those two.

Q. Those proportions are important, are they?

A. Well, you have to use certain proportions in order to accomplish Humphreys' disclosed results, yes.

Q. What are those proportions?

A. If you are relying on an air-cooled run back, it gen- [fol. 1114] erally works out on a shell still or most any kind of a still, that the area exposed to air cooling is larger than the heating surface.

Q. In the stills used by Humphreys the surface of the still is twice that of the cooling pipes, is it not?

A. Well, you do not heat over the whole surface of the still. You are not immersing it in a fire box.

Q. Humphreys does not say how much of the still he heats, does he?

A. No, but anybody skilled in ahe art would know that.

Q. He may use only one of the pipes?

A. Toward the end of the run.

Q. In that case, the cooling surface is only one quarter the surface of the still?

A. Well, that is a fact. I do not see that it has any particular bearing on the problem.

Q. No, but those proportions are supposed to be important, if I understand you?

A. No, the important thing is the disclosure of what you wish to do. In order to accomplish that, certain things are necessary; but given the disclosure there are many different ways of accomplishing essentially the same result.

Q. The accomplishment of it is simple after you know what you want to do, is that the idea?

A. That is the big point.

Q. And knowing that Dewar & Redwood wanted to get light vapors, lighter than they started with, and knowing that they returned some of the condensate, we would naturally assume that they returned the lighter condensate, is that right?

A. If you are willing to have that that way——

Q. I am not; I just want to know your position. It seems perfectly absurd to me.

A. The problem, as I pointed out, involved in returning a small amount of very heavy material is an extremely simply one. It is when we want eighty to ninety per cent of vapors condensed, which is far beyond what anyone previously dreamed of, that brought up these problems, of regulated partial cooling, the necessity of fractionation, and particularly the necessity of stripping.

Q. Humphreys went further than Dewar & Redwood in that respect, is that right?

A. Dewar & Redwood did not have it.

Q. They returned some, did they not?

[fol. 1115] A. Sure. So did many of the prior art stills.

Q. Pressure stills.

A. Well, I should not have attributed it to many; but some others of the pressure stills.

Q. What others?

A. Pielsticker had a similar very minor return in his little dome.

Q. What he would return would be the heavier ends, rather than the lighter ones, is that not true?

A. As comparing heavier rather than lighter, but that does not say they would be well stripped.

Q. No, but you would take out the heavier ends and let the lighter go through; even though it was not well done.

A. He would condense a very small amount of heavy ends, just as every still dome does, yes.

Q. And let the lighter ones go on?

The Master: Why did Dewar & Redwood put this chamber C-2 in between their pipe for the escape of vapors and their line to the condenser?

The Witness: I think it was primarily to stop entrainment, your Honor. That is the general purpose of such a still-head; to stop entrained drops from going through. I think that was the primary purpose of Pielsticker, as well.

The Master: Do they distinctly disclose anything further than that?

The Witness: No, they do mention that the incidental condensation shall be returned, to be sure.

Q. "Incidental"; that would be condensation by the hot walls, or rather transferred through the walls of C-2?

A. Yes.

Q. And that would be condensation at rather high temperature, would it not?

A. Well, those dead ends might get fairly cool.

Q. The stuff that went through in contact with parts that were not dead ends would condense? If there were dead ends and quite a lot of condensation taking place, they would not be dead; there would be circulation?

A. Yes, but you tend to pile up uncondensable gas there.

Q. So at least the cooler parts would tend to reflux then; wouldn't you have convection?

A. Yes, there would be some convection in there.

Q. And consequently circulation?

A. A certain amount of circulation.

[fol. 1116] Q. And the portions that came in contact with those walls stopping the circulation would become condensed?

A. The walls might be quite cool.

Q. If the vapors were circulating around there they would not be quite cool, would they?

A. That depends upon the circulation that you have.

Q. The cooler they get the less it circulates?

A. Hot vapors would be going up the center and the cool vapors coming down the side, but the tendency of gas to pile up there is a factor we still have to keep in mind. That gas in itself is lighter, so it would tend to work against those convection currents.

Q. It would go up to the upper part of the dome, would it not?

A. It might tend to reach more or less a state of balance.

Q. Then, when it got up to the top it would go out through the pipes in the top into the condenser?

A. I say there might be a balance in there. We very frequently have, on a dead end connection like that, a considerable collection of gas, and because of the cool condition a slow circulation, to be sure. We cannot stop all circulation.

Q. And you think that stillhead would be quite cool, do you?

A. That would depend largely on the method of operation. If you got substantial condensation; say, you put a water spray on it, or something of that kind, to operate by the crackling method, why, I think the ends would get pretty cool.

Q. You would return quite a lot of heavy material, would you not?

A. You would return heavier and intermediate material in that case.

Q. That would flow down through the vapors?

A. No, that would run down along the side- sof the stillhead.

Q. Then down through the neck?

A. The very short neck which is shown, yes.

Q. Mr. Wilson, in Day's Handbook of Petroleum Industry, Volume II, page 428, I find the following statement in regard to the Dewar & Redwood process: "Due to the production of comparatively large quantities of gasoline, which was practically unsalable at that time, the process was aban-

doned." Would you agree with that statement of Dr. Day's?

Mr. Wiles: I object to that statement as hearsay. The question of the practical use of the Dewar & Redwood process is a matter to be proved by witnesses, if at all.

The Master: It is a statement of facts—a plain question of facts. If the witness knows anything about it he can say so, and if he don't know anything about it, he can say no.

A. That is the only time I ever heard any such statement, and I know of nothing to make me believe it is true, although I don't know.

Q. Your opinion is, however, that the process would be impracticable, because they used a dirty stock?

A. It would be impractical for making gasoline on a large scale. He would, undoubtedly, make some gasoline, protecting his tubes as he did, and he could run along with dirty stock in a small still, but, of course, his scheme of protecting his bottoms is not a practical scheme for large scale operation.

Q. One of the things disclosed by the Burton patent, then, is how to determine whether a stock is dirty or clean, for this purpose?

A. No. Burton's patent discloses a clean stock—a stock which is clean, and which does permit large scale operations, to make commercial quantities of gasoline.

Q. And from that disclosure of the Burton patent, those skilled in the art, could operate on a large scale successfully?

[fol. 1117] A. If they used Burton's stock.

Q. And if they used Burton's process?

A. And Burton's process.

Q. If the Burton process were installed for California oil, would it be a success, in your opinion?

A. Well, there are so many elements involved there. Undoubtedly there are some California oils on which it would work, yes, but I wouldn't say it would work on all California oils. It would depend largely upon whether the stocks were substantially equivalent to Burton's gas oil stock, in their behavior.

Of course, California oils are characteristically high in sulphur, and it is difficult to run such oil in ordinary types of processes. The equipment is sometimes chromium plated

inside to avoid corrosive action, and while that process is successful for months, or a year or two, the corrosive chews up the equipment so rapidly that it eats up the profit, and some steps have to be taken to avoid that difficulty with California oils. The special difficulties with these oils are not so much concerned with cracking problems as it is with side reactions, on account of their high sulphur content.

Q. Then, with all the knowledge disclosed in the Burton patent, those skilled in the art might not be able to operate the process successfully on all oils?

A. Oh, no—certainly the Burton process will not operate successfully on all oils.

Q. On all oils boiling upwardly of five hundred degrees?

A. What do you mean by 500 degrees upwardly?

Q. What it means in the patent—around 500 to 750.

A. It won't operate on all oils that boil upward of 500 degrees. For instance, it will not operate on all shale oils that boil up in that range.

Q. Well, on all gas oils that boil upwards of 500 degrees?

A. I don't think it is certain that it would work on all gas oils, but I think it would operate on substantially all gas oils—I am quite sure that it would operate on gas oils from crudes such as Burton describes, which give lubricants and waxes, as well as gasoline and kerosene and gas oil.

Q. Do you know whether it would operate on gas oil from California?

A. Yes, it would undoubtedly operate on the gas oil from California.

Q. Is it necessary in the operation of a bulk pressure distillation system, in the manufacture of gasoline, to condense under pressure?

A. No, of course not.

Q. How much gain do you get in condensing under pressure, over what you get in not condensing under pressure?

A. About eight per cent of the gasoline, in a bulk pressure distillation process, in a still of the type of Burton's. That varies, depending upon the amount of gas produced, and the pressure employed.

Q. Did you determine the quality of gasoline made in the Burton process when condensing under pressure and in a similar process not condensing under pressure?

A. I made comparisons to determine as exactly as I possibly could, the losses created by either separating gas under

pressure, or separating the gas not under pressure, using the same stock, and everything else the same, in a small Burton still.

Q. How did you determine that loss?

A. It has been some little time since I carried that experiment out. As I recall it, we first measured the amount of gas which was produced per unit amount of distillate, as an average, in a Burton run. Then we took some of the distillate from which the gas had been separated under pres- [fol. 1118] sure in a bomb, and put in the bomb also, the volume of gas which corresponded to this amount of dry gas, which had been released. We then discharged the contents of the bomb through a long pipe into a receiver, and determined the volume which we were able to collect in the receiver, in that case, where the same quantity of gas separated from the liquid at atmospheric pressure, as compared with the amount when it was separated under higher pressure.

Q. How much gasoline is produced in the ordinary Burton process?

A. Gasoline from what?

Q. From gas oil.

A. Well, I think we averaged about 35 per cent.

Q. Then you would get eight per cent less if you did not condense under pressure?

A. Eight per cent. of the gasoline.

Q. Would that be 27 per cent. then?

A. No.

Q. What would it be?

A. That would be two and eight tenths per cent. less, or thirty-two and two tenths per cent., instead of thirty-five per cent.

Q. Now this gasoline, when you take it from the condenser, you run it through a coke tower, to let the gas escape?

A. The gas which still remains dissolved at the high pressure, yes.

Q. And that gas is sent through a recovery process?

A. Yes.

Q. And all the casinghead material removed from it, and returned to the gasoline?

A. Substantially all, yes.

Q. All that you can get out?

A. Yes.

Q. How much of the gasoline goes into that gas, in running it through the coke tower?

A. Three or four per cent.

Q. That is about half of what you gained by condensing under pressure, is it not?

A. That would, of course, be added to the other loss. No it isn't half of what we gained, because the amount we save [fol. 1119] by condensing under pressure is the amount which would be carried off by the amount of gas which would be separated at the higher pressure. In addition to that there is a certain amount which we can not save even by condensing under pressure, corresponding to the amount of gas which dissolves in the liquid in the drum, and that which we do not save is what we later have to pick up in some other way.

Q. In the gas absorption tower?

A. In the gas absorption tower, yes.

Q. All the gasoline that you would lose by not condensing under pressure would go into the tail gases, would they?

A. What do you mean by tail gases?

Q. The gases coming out through the receiving tank.

A. Yes.

Q. And you could recover all those in a gasoline recovery process?

A. Not in ours.

Q. You could recover those gasoline constituents from the gas, could you not?

A. They could be recovered by a much more expensive type of process than we now use.

Q. What would be the cost of recovery, per gallon?

A. I haven't any precise figures in mind. I know we looked up the cost of a charcoal plant, but I don't think that would be the type of plant to use for that purpose. It would be a matter of a few cents a gallon. I cannot tell you exactly what it would be.

Q. You don't know how much more expensive it would be, then?

A. Not exactly. There is no question but what it would be more expensive, though, because our present plan is to scrub with heavy naphtha, which involves no step of distillation—because the product goes into the gasoline, direct. But, however, if we were going to handle three times as

much gas, which is approximately what we would have to do if we didn't separate our high line gas under pressure, we would have to put in an oil scrubbing system, and then distill the light material out of the oil or charcoal, and mix it back with the gasoline, which is a much more expensive proposition.

Q. But by that system you could recover all of the losses?

A. Yes, certainly. Burton's scheme is simply a simpler method of accomplishing the same thing.

The Master: Would the amount recovered be as much as the expense of recovery?

[fol. 1120] The Witness: Yes, I think it would pay to scrub the additional gas to recover the light ends if you did not separate under pressure. The margin would not be anything like as large as at present where condensing under pressure costs you practically nothing.

Q. That is the system that is used profitably in extracting casinghead gas from natural gas, is it not?

A. Yes.

Q. Returning to the Dewar & Redwood patent, petitioner's exhibit 103, I understood from your testimony yesterday that a certain quantity of the heavy material would condense in the stillhead C-2, and flow down through the neck into the retort C, is that correct?

A. Yes, if you do not imply by the term "heavy" anything that is well fractionated. It would be more heavy than light, of course.

Q. There would be some light, you think, with that?

A. There would be some light with that heavy?

Q. And in going down through that neck, would some of the light materials distill off?

A. Well, your contact is very slight. If the wall were cool, you might find that that neck was doing some condensing itself, but I suppose a very small amount might distill off; almost an infinitesimal amount.

Q. That action would be the same even though in lesser degree than in the Humphreys runback, is that so?

A. The stripping of a small amount of light ends from a small amount of total condensate in a very ineffective manner seems to me quite different from the problem of strip-

ping a very large amount of light ends from a very large amount of partial condensate.

Q. The action would be the same only in not so great a degree?

A. Well, it is really a different order of magnitude entirely.

Q. With a different degree; one does it more than the other, is that right?

A. It is a different order of magnitude. That is the clearest way I can express it. That is, it is such a totally different thing to take a few per cent of heavier ends and get out a little bit of light stuff in a six inch tube, as compared with what Humphreys did, that I do not consider it a mere matter of degree.

The Master: Would the small neck perform all of the func-
[fol. 1121] tions that are performed by the long runback of Humphreys?

The Witness: No, it would not perform the function. That is the trouble.

Q. You mean it would not distill off as much?

A. If you had Humphreys' runback going down through there it wouldn't touch it.

Q. How is that again?

A. If you had a runback such as Humphreys had running down that neck, it would not touch it as far as distilling off light ends is concerned; the surface being so much smaller.

Q. I do not know quite what you mean by having Humphreys' runback down through that neck. You mean all the liquid that runs down through Humphreys' runback?

A. I mean if you had an amount of liquid even comparable to that of Humphreys', that neck would not perform Humphreys' function at all, of getting out the gasoline.

Q. It would not get out any of it at all?

A. I suppose theoretically you would get out a trace, but that does not solve the problem.

The Master: In other words, is Humphreys' runback the small neck leading to the small chamber C-2 in Figure 1, of Dewar & Redwood?

The Witness: No, it certainly is not.

Q. Across the stillhead C you would get some gas there, would you not?

A. You would not get any stripping action there. You would get condensing action. Of course, Humphreys' runback had several functions as I pointed out.

Q. You would not get any stripping at all?

A. In the stillhead?

Q. In the stillhead.

A. I do not think so, no.

Q. It could not be designed to give stripping?

A. Well, of course it could be designed to give stripping.

Q. What changes would be made?

A. Build it like Humphreys' runback.

Q. You are not using the Humphreys runback today, are you?

A. We certainly are.

Q. Just as is shown in this patent?

A. Not just as is shown in this patent. Well, we are, yes. We have made some additions to it to take care of the larger still, but we are using Humphreys' runback and the only stripping that we get, is what we get from Humphreys' runback. We get a little additional condensation, I mean, on many of our stills.

The Master: You cannot read into the prior disclosure of Dewar & Redwood the subsequent art of Humphreys or afterwards?

Mr. Rauber: I do not think I am trying to do that. I want to find out what his idea of the operation of Dewar & Redwood is. He says there was some stripping in that neck, the longer the neck the more the stripping, is that right?

The Witness: Yes.

Q. The purpose of this Humphreys runback is to return the heavier material so that it can be re-cracked, is that right?

A. Yes.

Q. That expedient was well known in the atmospheric pressure distillation art, was it not?

A. There is quite a bit of difference there. I have, of course, discussed in detail the dropping back theory of cracking and the errors and incorrectness of that theory, and the absurd designs that it led to.

Of course, the dropping back theory which was in the prior art very clearly, was that something in the sudden heating of the drop as it came back, or the sudden flash vaporizing

of those drops was responsible for cracking, and I have pointed out that that was not true. And that cracking was [fol. 1122] really taking place in the main body of the oil.

Now, it probably is true that a small portion of the poorly fractionated unstripped condensate which came back in that type of still would remain dissolved in the oil and would, in the course of time, be cracked; but that is quite different from what they were aiming at, or their disclosure, and it is unquestionably true that substantially all the cracking takes place in the main body of the oil in the still.

Q. And in Humphreys the idea was to return the heavier products that had been carried off, return it to the still so that they would have another chance to crack, is that right?

A. Heavier products——

Q. The vapors.

A. Well, stripped from the light products, yes. That is an important element.

Q. You say that expedient was not known in the prior art?

A. What prior art?

Q. I will refer you particularly to petitioner's exihbit 118, page 1487, the bottom line. It starts, "In this manner." Would you read that line?

A. "In this manner also"——

The Master: Let us find out what the manner was.

Mr. Rauber: How is that?

The Master: Let us find out what the manner was; you said "In this manner". Let us find out what the manner was. Where is the antecedent description of the manner?

The Witness: I notice that one important thing about the manner is in the middle of that paragraph, where it says: "Care must be taken to ascertain whether no pressure exists in the boiler."

Which, of course, distinguishes it quite clearly from Humphreys' operation.

As I pointed out, when you come to bulk pressure distillation, you have got many very serious problems arising, in direct connection with the runback.

Q. I understand the manner that he is referring to there is the ordinary atmospheric pressure distillation in which the heavy products are returned to the still?

[fol. 1123] A. Yes.

Q. And he states the purpose why they are returned to the still.

A. Yes, he states that they are put back for cracking. That was what practically all this prior art stated, these heavier products are returned to crack.

Of course, when he talks about heavier porducts, you understand he is talking about material in the lubricating oil range. That is quite different from what Humphreys is talking about.

Q. That reference to "In this manner"; will you briefly describe what that refers to?

The Master: This refers to dehydration of the oil at the beginning of the run, does it not, in Aisinman on page 1487.

Mr. Rauber: I think it refers to the cracking operation.

The Master: Well, begin back at that paragraph: "Now, as regards the running of the distillation process proper, the same depends not only upon the nature of the plant but in part also upon the property of the material to be worked." Then he describes this distillation for complete dehydration of the oil.

The Witness: I cannot find any statement at all as to how these heavier products are supposed to be condensed. I suppose it is the customary few per cent that condenses in the stillhead. He does not describe any particular means. Maybe he had a still with a cold top like they had there, which of course is a very foolish arrangement.

The Master: Was this description of this process distilling under pressure?

The Witness: No.

Mr. Rauber: No; I refer to that—atmospheric pressure.

The Witness: No pressure at all. Just substantially the ordinary coke still practice.

Q. That is the description in connection with the reflux condenser shown on page 1445, Fig. 3, is it not?

A. I will check that up with the translation.

Q. Or page 1447—The condenser being shown as C.

A. I rather think he is referring to a device of that type. Now, it is hard to make sure, and I cannot quite figure out just how the figure 3 works. There seems to be no indication how the vapors get——

The Master: I am looking at figure 4 on page 1447. Is there another figure there?

The Witness: He referred me first to figure 3.

Q. Figure 4 shows it better, I think.

[fol. 1124] A. It does not show any details at all. From figure 4 I see no indication of a return to the boiler. In the description of figure 3 there is some such indication, but I cannot see what the path of the vapor is in figure 3.

The Master: What is that dark line in figure 4?

Mr. Rauber: Going down through the brickwork.

The Master: No, not going down through the brickwork at all. (Indicates to witness). This dark line there.

The Witness: Well, you cannot see just what that comes from. There is an outlet pipe A there.

The Master: It is neither letter nor numeral. Is there any description of that—what that line is?

Mr. Rauber: I think there is. It may be hard to find here.

The Master: I am referring to that black line there (indicating to Mr. Rauber).

Q. That is the only way it could get back into the still, by running back through a pipe, is it not?

A. Yes, and in this arrangement it cannot do as in Humphreys—flow back through the same passageway, thus giving it a chance to be stripped. It would have to go back through a separate pipe.

Q. Any stripping would take place in that air-cooled condenser, would it not?

A. Any stripping that it got would have to take place there.

Q. As the liquid trickled down the walls and the gas went by there would be a stripping, would there not?

A. There would be a certain amount of stripping take place. I cannot see, though, what the course of his vapors is.

Q. I think they run into the bottom of his condenser and then pass upwardly.

A. How do they get to the outlet?

Q. It is a double wall condenser, I suppose.

The Master: His vapors pass through the small pipe *e* into the bottom of the separator C, do they not?

Mr. Rauber: Into the annular chamber.

The Master: He calls it an annular chamber, but it is a separator, isn't it—a fractionator, a fractionating chamber?

The Witness: It says, "the light oil vapors enter at *c* into the inner space of the dephlegmator".

Q. The space is within the two cylindrical walls, is it not?

A. No, because that is where they first go.

Q. And then they pass upwardly?

[fol. 1124½] A. Yes.

Q. And out through the top?

A. No, they do not pass out through the top. They pass through at the line——

Q. (Interposing) *d* on page 1447.

A. It is D in the text.

Q. D is a manhole.

A. I mean the text says that they go from there to the cooler by way of a four inch overflow pipe at D.

Q. That apparently should be *d*.

A. But there is apparently a reflux drawn off from the inside chamber there, which of course would not be subjected to any stripping action as far as I can see.

Q. What reflux that is drawn off are you referring to?

A. That center one shows in the plan view, right in the center; the small outlet right in the very center of the condenser.

Q. But in passing upwardly through that condenser C the heavy vapors would be condensed, would they not?

A. Yes——

Q. And trickle down the walls of the cylinder?

A. Lubricating oil.

Q. And trickle down the walls of the cylinder?

A. Yes.

Q. And come in contact with the hot vapors going up?

A. Yes.

Q. And be stripped to some extent at least?

A. They would be stripped to some extent.

Q. More than in Redwood?

A. Well, that depends on the relative proportions.

Q. As shown in the drawing?

A. I should judge they would be stripped more than in Redwood, yes.

Q. And that is the heavy material which he is returning to the still, is it not?

A. Yes. Of course, I do not want to imply that his heavier material is anything like or faintly resembles Humphreys'.

Q. No, but leaving that aside for the moment, it is heavy material?

A. It is heavy material, yes.

Q. And that is the manner that is referred to at the bottom of page 1447?

A. But he has some additional condensation. He returns [fol. 1125] without any stripping action, apparently, through this small pipe *e* in the center of the device.

Q. Oh, some condensate running down that pipe e?

A. Yes.

Q. That material would come in contact with the hot vapors, too, would it not?

A. Well, it would not be countercurrent to them at all.

Q. The liquid flowing down the pipe *e* would not be countercurrent to the vapors flowing up?

A. No, because the vapors apparently must flow down through there if they flow through there at all.

Q. They flow from that large cracking chamber up to C, do they not?

A. I am talking about the condensation in the inside part of the condenser, and that is where your lighter vapors pass, and therefore that is the condensate that really needs the stripping most.

Q. Do you mean the part near the inner wall *m* of figure 3 on page 1445?

A. The part inside of that.

Q. That is where the air goes up through the center?

A. No. He says in this description page 1483:

"The condensate flows through the pipes *b* and *e* provided at the bottom of the dephlegmator back in the boiler, whereas the light oil vapors enter at *c* into the inner space of the dephlegmator, reaching from there the cooler by way of a four inch overflow pipe at D."

In other words, that indicates the vapors pass into this inner space of the dephlegmator.

The Master: The very essence of Humhpreys is pressure and I don't see how it can be compared with any distillation at atmospheric pressure, at all.

The Witness: Humphreys solved a new problem in returning kerosene vapors to a pressure still and cracking them, and that is the big problem, as I pointed out.

The Master: The thing he specified particularly, is that, "There shall be a valve, 5, that used to control pressure in the pipes, 4, and to maintain a pressure of four atmospheres."

Q. That is, of course an expedient that was old in atmospheric pressure distillation—the same action.

A. Returning kerosene for further cracking was not known in any art, that I know of.

[fol. 1126] Q. On page 1487 and 1488, he states that they return the heavy condensates.

A. That is a very simple problem. I mean that, in the old art, they simply returned the heavy lubricating fractions for further cracking.

Q. And that is exactly what Humphreys did?

A. It is almost the exact opposite of what Humphreys did. Humphreys' problem was to return and recrack kerosene, and I pointed out that that presented some very serious problems. He accomplished it by doing it under pressure in a fractionating, and stripping condenser, by which he returned the kerosene for recracking.

Q. Does Humphreys mention returning kerosene, in his patent?

The Master: Here is the very essence of it, on page 2, of Humphreys: "Such fractional condensation as occurs in the runback, 10, however, should evidently be under pressure, for the carrying out of my process, as, otherwise, there would be considerable difficulty in returning the condensed fraction to the still, for further treatment therein."

Now, there is no valve, or pressure in Aisinman—so how are they comparable?

Mr. Rauber: The action is just the same, whether there is pressure or not. You condense out the heavier and strip out the light.

The Master: In other words, you say—The action might possibly be the same, but the process is not the same. Do you mean by the action—you mean the result?

Mr. Rauber: Merely the return of the heavier products for recracking.

The Master: Humphreys says if you had no pressure on the still they would not return.

Q. In running the ordinary distillation to produce various products, from which Humphreys gets his gas oil, will you give me the sequence of the products he gets off, starting with gasoline?

A. Do you mean the operation of the ordinary coking stills?

Q. Yes.

A. As I pointed out, the primary cuts that you can get will differ in practice. You will get gasoline, kerosene, gas oil, paraffin distillate—which is frequently divided into light and heavy paraffin distillates—the slop cut and the still wax.

[fol. 1127] Q. What is the difference between the light paraffin distillate and the heavy paraffin distillate?

A. The viscosity and the gravity of the cuts—they are both part of the paraffin distillate.

Q. And what is done with those distillates? Is there any further treatment given them?

A. I have gone through that in detail, in my testimony. Do you want me to go over it again?

Q. You chill out the wax, do you?

A. Chill out the wax and put the oil through the distilling process.

Q. What temperatures do you use in chilling out the wax?

A. It depends on what cold test oil you wish to produce, and it also depends on the crude.

Q. What is the temperature usually used—what range of temperatures?

A. The range of temperatures ordinarily employed, is from zero to 25 degrees Fahrenheit.

Q. What determines the temperature—the amount of wax in the distillate?

A. No, it is largely a question of the kind of stock you want to make. If you are content to make a stock which will solidify at 25 degrees Fahrenheit, then you don't need to use such low temperatures.

Q. What is the highest temperature at which you could freeze out the wax?

A. As it stands there in the bottle there is some wax separated out.

Q. Will the quantity of liquid distillate have any influence on that?

A. Some.

Q. What influence will it have?

A. Well, the larger amount of wax free distillate, the lower will be the solidifying temperature. The larger the amount of the heavier distillate, and the larger the amount of wax the oil contains, the higher will be the temperature at which wax begins to separate. Some wax will generally separate out at temperatures around a hundred.

Q. A hundred degrees Fahrenheit?

A. Yes, sir.

Q. Would they contain much gas oil?

A. What?

Q. Or of liquid distillates—those that freeze out at 100 degrees, and above?

[fol. 1128] Mr. Wiles: Do you mean primary, or secondary gas oils?

Q. Secondary gas oils?

A. Yes.

Q. Could you freeze it out at that temperature if it contained much primary gas oil?

A. You would get some separation of wax—I don't mean to say that you would separate out enough to make a wax-free product, but you will get some separation of wax.

Q. At what temperature?

A. As high as a hundred degrees.

Q. But not higher?

A. I don't know the exact temperature—I can't give you within five degrees—or I can't give you within ten degrees—in that vicinity—all that I particularly know is that those cuts characteristically contain considerable amounts of wax some of which will separate at room temperature, and some above.

Q. Will much of it separate out at 150 degrees Fahrenheit?

A. We are talking, now, about American petroleum—not about shale oil?

Q. Yes.

A. No, I don't think you can get anything at 150 Fahrenheit—from any paraffin distillate.

Q. Whether or not it contained much gas oil?

A. No. The wax, itself, is melted before you reach any such temperature as that.

Q. What is the melting point?

A. The ordinary mixture which we get out, melts at around 125.

Q. Is that a mixture of paraffin wax——

A. No, pure wax, although it is a mixture of low melting wax and high melting wax. If you actually separate the individual cuts of wax, which is, of course, very difficult to do, you would find that if it probably covered a range of melting points from 110 to 140 or 150 degrees F.

Q. Does shale oil wax melt at a higher temperature than wax from American petroleum?

A. As a rule, yes.

Q. How much higher?

A. I have heard tales of shale oil wax that melted at around 170, but I do not have any first-hand knowledge of that.

Q. You do not know, in other words?

[fol. 1129] A. No.

Mr. Rauber: Mr. Wiles, have you that exhibit of the Barbet drawing?

Mr. Wiles: Yes.

Mr. Rauber: I have got a photostat but I would like to run through that again.

The Witness: What Barbet exhibit is that, by the way?

The Master: He has sent for it.

Mr. Wiles: I have sent for it. Mr. Dawson has gone after it.

The Witness: I mean the exhibit numbers of Barbet in the book. Oh, it is number 136 and 141, I remember.

Q. Now, in the course of the oil going down through that system there, it passes down through the pipe P, is that correct; the part you have marked "a light reduced crude"?

A. Yes, the pipe to the left of P.

Q. That is a crude out of which all of the gasoline has been stripped, is that right?

A. Substantially all, yes.

Q. That is the purpose of it?

A. Yes.

Q. Then it goes down through a heater G?

A. Yes.

Q. And up through the pipe into the side of the tower, T?

A. Yes.

Q. And from there the vapors go up through the tower——

A. Excuse me, that tower is H not T.

Q. Tower H. The vapors go up through the plates T4 and T3?

A. Yes.

Q. And the heavier parts of those vapors are condensed, are they not?

A. Yes.

Q. And flow back on the plates?

A. Yes.

Q. The material that flows out through the pipe S into the proof bottle O, that is a very light kerosene, is it not?

A. Well, it is described as a light kerosene.

Q. Light kerosene. And the part flowing out through the pipe S′, into proof bottle O′ is an intermediate kerosene, is it not?

A. Yes.

Q. Then the part flowing out through the pipe S_2 into the proof bottle O_2 is a heavy kerosene, is it not?

[fol. 1130] A. Yes.

Q. That is supposed to be, or is stripped of the lighter portions of the kerosene?

A. Not perfectly clean.

Q. As near as he can?

A. Well, no, he could do better stripping than that if he had a steam stripper. That is, a column like this, with vapors passing by is not as good as a steam stripping column, but the kerosene is as well stripped as possible in this type of apparatus; yes.

Q. It would have a rather low content of light ends, would it not?

A. Yes.

Q. In your direct testimony you said that by including a large quantity of light ends in kerosene you could put in a greater quantity of the heavier ends, could you not?

A. Yes.

Q. So, by excluding these light ends in the proof bottle O_8, you would not be able to put in as many of the heavier ends, those than ran into the gas oil fraction?

A. Not if you wanted to make a high quality kerosene; but this is not supposed to be a high quality kerosene.

Q. It is supposed to be a lighting oil or illuminating oil, is it not? It is so described, is it not?

A. I think so. Calling it heavy kerosene is an indication that it is not supposed to be a high quality product.

Q. No, but it is supposed to be kerosene?

A. Yes.

Q. And having very little of the lighter ends of kerosene it could not have many of the kerosenes that are found in gas oil?

A. Why, certainly it could. That depends upon how low——

Q. In illuminating oil?

A. It depends upon how low a grade of illuminant you can stand. It depends upon the lamps and how it is used.

Q. Then, that statement that you can include a large quantity of heavier hydrocarbons if you increase the quantity of lighter ones is not true?

A. It is true, to make an oil of given quality.

Q. But if it is a kerosene of any type at all, rather than a gas oil, and has but very little of the light ends, it cannot have very many of the heavy ends, is that right?

A. No, that it not true.

[fol. 1131] Q. It would be a gas oil then and not an illuminating oil, would it not?

A. Well, as I pointed out, a great portion of the gas oil cut in the old times was used for illuminating oil.

Q. Because they had a great deal of the light ends in, did they not?

A. In part.

Q. If, as you say——

A. (continued). If they put a lot of the light ends in, they would get a good quality of illuminating oil, even with the gas oil. If they left out the light ends, however, it would still be kerosene which would come within this full range.

Q. Do you mean to say that you can take out the light ends of a kerosene and the intermediate portions, take the heavier parts of your kerosene distillate and include in it practically all of the gas oil and still have kerosene?

A. You would have something which would be illuminating oil. It would not meet our present specifications today, but it would still be illuminating oil.

Q. Could you sell it as illuminating oil?

A. I don't know whether we could or not, but we probably could. I have seen some pretty poor kerosenes on the market.

Q. That would be practically gas oil?

A. That would certainly contain very considerable quantities of gas oil.

Q. No light ends or very few?

A. It would have some light ends. As I say, this column does not give one hundred per cent stripping action. With good fractionation you would get a distillate such as you assumed, but the stripping of these intermediate cuts is not so good.

Q. But you would have less of the light ends in that proof bottle O_2 than if you had not taken out the lighter portions in O and O'?

A. Yes.

Q. So, if it is illuminating oil it cannot contain all the gas oil?

A. Probably some remained. It might not.

Q. You would put all of your gas oil in your kerosene?

A. Yes, they ordinarily did.

Q. They ordinarily did that?

A. Very frequently when they are running for a maximum yield of kerosene they left the light ends of gas oil in the kerosene, especially when dealing with the preliminary, primary cuts.

Q. Then your statement that you can increase the quantity of the heavy ends that you put into kerosene by increasing the light ends does not mean anything?

A. It certainly does.

Q. That you can put them all in without those light ends?

A. It is a question of whether or not you are going to forget about quality. If you forget about quality, and admittedly make a poor quality of oil for certain, special purposes, you can put in very different things, and very much different rules apply from what I gave to make an oil which would meet certain standards of quality.

Q. Did you ever know of any illuminating oil that contained all of the gas oil fraction and particularly without the light ends?

A. Yes, our Mineral Seal meets those specifications almost exactly.

Q. That is a lighting oil?

A. Yes.

[fol. 1132] Q. That paraffin distillate to which Mr. Wiles refers is on the plates H and not on the plates H'; is that true?

A. Well, if they are boiled up from the plates H, they pass up into the upper part of the column; the vapors are rising.

Q. Unless they are condensed?

A. Yes.

Q. If Barbet takes nothing higher than the kerosene distillate through that pipe S'', then of course anything heavier than that goes down back into the boiler M, does it not?

A. Anything heavier than the heavy kerosene cut, would pass down the column, but would not necessarily go back to the boiler M.

Q. Where would it go?

A. It might be reboiled on the plates in the lower part of H.

Q. It could not get up above the plates H' and flow out?

A. Why not?

Q. Not if it is heavier than the material that you take out?

A. It becomes the heavier part of that material. Do you mean heavier than the heaviest material that you take out?

Q. Yes. If you are taking your kerosene out there you cannot have gas oil vapors up above that plate, can you?

A. You are drawing a distinction between kerosene and gas oil which is not justified. As I say, we make one illuminating oil, namely, mineral seal oil, which is really a highly refined gas oil cut. You cannot burn it in all kinds of lamps, and you have to treat it with a great deal of acid in order to make it a good illuminating oil. Gas oil is simply what is left over after you make all the kerosene you can sell.

Q. And there is some left over usually?

A. When we are running today for a relatively small yield of kerosene, yes. When we are running for a maximum yield of kerosene there very frequently is not any left over, at least so far as the primary cuts are concerned—up to that which is required to go into the paraffin distillate.

Q. All you would have coming down there would be a light paraffin distillate, the next cut above paraffin distillate?

A. You vaporize all the illuminants, and some paraffin distillate. There is nothing in between those two products in what Barbet is considering.

Q. He says that the paraffin distillate is vaporized on the plates H, the lower plates; is that right?

A. Yes.

Q. But he does not imply they go into that kerosene, does he?

A. He seems to imply that. I do not say that necessarily followed. I would say that he implied in that statement that the kerosene had some of the paraffin distillate. It is a low grade product distinctly. I pointed out that when he had only two kerosene cuts, you will recall, he says there are fifteen or twenty per cent of heavier oils, or ten or fifteen per cent of heavier oils, in this heavy kero- [fol. 1133] sene. When he makes three cuts I would expect to find even more of the heavier oils in his heavy kerosene cut.

Q. And he could put more of the gas oil in that cut even though he stripped out the light ends and put them in O and O'?

A. If he drops his quality standards.

Q. Then it no longer continues to be kerosene, of course.

A. Why, sure, you can call anything kerosene.

Q. You can call gas oil or paraffin distillate kerosene, then?

A. Yes.

Q. It is a mere matter of calling it?

A. The real question is, is there an intermediate cut in this process, between heavy kerosene and light paraffin distillate, from Barbet's disclosure and method of operation, there is clearly no intermediate cut between those two.

Q. Did they sell Mineral Seal oil as an illuminating oil in 1902?

A. I do not know.

Q. You do not know whether it came in at that time or later as an illuminating oil?

A. No, I do not. It does illustrate the fact, however, that you can burn very heavy products in special types of equipment.

Q. That is described as a heavy illuminating oil and not a low-grade illuminating oil, isn't it?

A. I pointed out in another part of Barbet's disclosures his statement that when he only made two illuminating oils the heavy illuminant was a low-grade illuminant. I do not know whether that is his exact wording, but anyway it contained a considerable percentage of heavier oils.

Q. Can you point that out to me?

A. It is in my direct. He says on page 1747, Exhibit 139——

Q. That is not the same patent.

A. No, Exhibit 139.

Q. Yes, but where is that?

A. In speaking of the separation of lamp petroleum near the top of p. 1747, he says, "In general two classes only are required: 1, a so-called high grade petroleum of a density less than .800 and containing no heavy oils; 2, a more common petroleum exceeding .800 in density and in which there is sometimes 15 per cent or more of heavy oils whose presence is detrimental to the brilliancy of the flame."

[fol. 1134] Q. That No. 2 would correspond to the kerosene now in use?

A. No, I think No. 1 would be much closer to the kerosene now in use, although I am not sure.

Q. Does the kerosene now in use have a density of .800?

A. Of course you are talking about shale products here and you cannot carry over your density figures from shale products to petroleum products and have it correspond to the same quality, and the same behaviour in burning.

Q. Does he say this is for shale oil?

A. I do not believe he does say definitely. He calls it petroleum, of course. I do not believe he does say definitely it is shale oil, but——

Q. Considerable quantities of American petroleum are sold in France at the present time, aren't they?

A. Crude petroleum?

Q. Petroleum products.

A. But this is crude petroleum he is talking about.

Q. Yes, crude petroleum. Are they sold in France—imported?

A. Not that I know of.

Q. You do not know whether they are or not?

A. American crude petroleums sold in France? I should be surprised if they were, although they may have been.

Q. And you do not know whether any shale product was sold there or not?

A. Well, I think there is not much question but what shale products were sold.

Q. Where did they come from?

A. Scotland.

Q. You feel quite sure there were shales there, though?

A. Shale products sold there, yes.

Q. But not crude shale oil?

A. Well, I don't know about that.

Q. You do not know whether this is shale petroleum or American petroleum?

A. That is exactly what I have said. I don't know. I am assuming that he is using the word in the European sense, but I don't know.

Q. Don't they ever speak of American petroleum as "petroleum" over in Europe?

A. I think their practice is specifically to designate when they do not use the common acceptance of the word. They [fol. 1135] say this is American petroleum or Baku petroleum, or something of that kind, to distinguish it from the European use of the word.

Q. That No. 2 product on Page 1747 you say does not correspond to the present-day kerosene?

A. I don't know. No, that does not correspond to the present-day kerosene, with the 15 per cent. of heavy oils, which impairs the brilliance of the flame.

Q. That patent came out eight years later than the other, didn't it?

The Master: Which patent, and which is "the other"?

Q. The patent to which you last referred on page 1747 came out six years later than the patent in Exhibit 136?

A. Yes, six years later.

Q. And they are entirely different types of patents, are they not?

A. Yes.

Q. I gather from your question that any vapors that are too heavy to go into that burning oil in O_1 must go down to the boiler M through the plates H; is that correct? There is a reflux there, is there not?

A. There is a reflux there, yes. What would happen, of course, is that a fractionating action would take place on those plates. While it might start down the products light enough to go into kerosene would be vaporized, but the products next heavier than the heaviest kerosene cut, would go on down to the still.

Q. And in the ordinary distillation that next heavier cut is gas oil or light paraffin distillate; is that correct?

A. It is either gas oil or light paraffin distillate.

Q. That goes down into the boiler M and from there goes through that——

A. (Interposing.) Oh, no.

Q. (continuing.) Through that pipe to the cracking chamber P; is that correct?

A. No, that is not correct.

Q. Where does it go?

A. Immediately the reflux hits the hot body of oil in M the light products are vaporized and distilled off through the line L, the heat exchanger G, and the cooler K.

Q. The patent states that only a small portion of those are drawn off, does it not?

A. I do not think so. Where do you refer to?

[fol. 1136] Q. On page 1691, in the fourth paragraph, it says, "Take up in L a certain quantity of vapor for the boiler G."

A. That is far different from saying that you take only a small amount.

Q. Would it make any difference if he said, "Take a small amount"?

A. Not particularly, so far as my ultimate conclusion is concerned.

Q. Then I refer you to his American patent, for the same invention, and I refer to page 1776, lines 16 to 19. You will agree that he states, there, that but a small part of the vapors are drawn off through L, to heat boiler G, or heater G?

A. Yes, he says a small portion of the vapors may be utilized.

Q. And that would be a composite of all the vapors going up from the boiler, M, would it not? There is no selection of vapors after that heater G, is there?

A. Yes, there is, because of the fact that only materials of a certain density can get down into the boiler M. In other words, they have been separated before they get into the boiler, M.

Q. It has got to be something, at least heavier than that?

A. Yes.

Q. The vapors must be heavier?

A. Yes.

Q. So that it will be the heaviest vapors produced in the system, that go through the valve, L, into the heater, G?

A. It will be the lightest vapors in the still M—it will be heavier than what is taken out here (indicating on diagram) but it will be but a sample of the vapors from the still M, which represents the lightest materials which enter still M.

Q. Now, the lighter vapors that come out of that still M, are what go into the kerosene draw-offs, 6, 7, and 8—isn't that right? Anything that is too heavy to go into those, doesn't get out of that system, does it—except a small quantity drawn off into heater G.

A. Now, you have two questions there. Do you want to cancel the first one? I am not clear as to what you mean.

Q. You have certain vapors going off from boiler M?

A. Yes.

Q. Well, the only outlet for those vapors is into the drawoff, 6, 7, and 8—there is no other escape, is there?

A. Yes, they can go out through L and G.

[fol. 1137] Q. Just those places?

A. The lighter products go into 6, 7, and 8, and the next heavier products go out through G.

Q. Oh, no—that part which goes out through G, you told me was a composite of all the vapors leaving M.

A. Exactly, but those are the products which are next heavier to those that come out in H.

Q. It contains everything that might be vaporized in M?

A. Yes, but the things that might be vaporized in M are the next heavier materials.

Q. If they were the next heavier materials, you wouldn't need those plates H, would you?

A. Those plates, H, are needed to make sure that no——

Q. It returns the heaviest part of this, back to the still M.

A. The next heavier constituents.

Q. If it contains the next heavier constituents, you wouldn't need those plates, H, would you?

A. Yes, in order to strip the light ends.

Q. The parts which you put into 6, 7, and 8?

A. Yes, you would need that, even if it was only stuff that was heavier that kerosene in that liquid, you would need the plates, H, to strip the kerosene, before the liquid ran down into the boiler.

Q. And your idea is that everything that is vaporized in M, goes back on those plates H, and is vaporized again?

A. What was that? Just how do you mean it comes back? Anything that is vaporized in M comes off as vapor.

Q. And that all goes through L—everything that is vaporized there—none of these vapors get back to M?

A. No, some of the vapors go up through the vapor spaces onto the plates H.

Q. And then are condensed and returned to M?

A. Yes, sir.

Q. And those vapors after being returned to M, go through that connecting pipe, over to the cracking chamber P, don't they?

A. No, not until you have driven off the lighter material —of what reflux goes down to the still, you vaporize the lighter portion, and that is what passes out through L and G.

Q. That is returned, out of the reflux that falls down to the still and vaporizes the lightest portion and that is what passes out through——

A. L and G?

[fol. 1138] Q. L and G. It takes the vapors from all of those vapors going out, which contains the heavier portions?

A. No, it only takes part of the vapors.

Q. It selectively takes out the lighter and leaves back part of the ——

A. The place where it selects is in the still. In the still is the only place; it vaporizes the lightest ends of what comes down this tower (indicating). By the time it gets here (indicating), the mixture of heavy paraffin distillate and the heavier ends are vaporized when it strikes those stills. When the reflux strikes the still the lightest materials along with some heavy ends that are vaporized, pass out through L and G, which mixture as I have pointed out, is a paraffin distillate.

Q. Just the lightest are vaporized in M and N?

A. The next——

Q. Answer that question; is that correct?

A. Just the lightest?

Q. Yes. That is, they go up in vapor from M?

A. No, you have some of the heavier as well, mainly lighter.

Q. Those are heavier than are drawn off through L, along with the lighter?

A. Yes, a little of the heavier along with much of the lighter.

Q. Exactly the same proportions that are formed in the still M, is that not correct?

A. Yes. But that is not the proportion in which they enter the still.

The Master: What is the bulk of the material in M?

The Witness: Heavy paraffin distillate and residuum.

The Master: The place where it is charged in M is not shown on the diagram?

The Witness: It comes into M through here (indicating).

The Master: Where?

The Witness: Here (indicating) comes your reduced crude, down through the pipe to the left of P, through the reboiler G where it is partly vaporized. The illuminating vapors are the main things vaporized, and rise through the column and the reduced crude falls through the column, being virtually stripped of its lighter ends, and then falls down into M and the next lightest portion of the oil that goes into M is of course immediately revaporized, leaving in M the heavier portion, mainly the heavy paraffin distillate and reduced crude.

[fol. 1139] Q. The liquid falls down into M through the pipes indicated in dotted lines, is that not right?

The Master: I do not get that at all.

Mr. Wiles: Trace that again, Mr. Wilson.

The Master: I do not get that at all. You have got a line here of reduced crude.

The Witness: Light reduced crude coming down through the line——

The Master: P.

The Witness: To the left of P.

Mr. Wiles: Light reduced crude, just the gasoline is off of it; everything else is in it.

The Witness: Then that goes up through this vapor heat exchanger.

The Master: Is that cold or hot?

The Witness: It is hot.

The Master: Where is it heated?

The Witness: By the vapors from the still M.

Q. It is heated before it reaches G, is it not?

A. Yes, it has been heated to some extent before that.

The Master: Oh, part of the apparatus is not shown.

The Witness: Part of the apparatus is not shown. The feed has already been heated hot enough to drive off the

gasoline. Then you get the oil hot enough to drive off the kerosene, so when you come in here you have got the kerosene vaporized.

The Master: But the heat is derived from M?

The Witness: By the heat which M gives off, that is correct.

Q. And the liquid flowing down those pipes flows through pipes represented in dotted lines?

A. How is that?

Q. The liquid flowing down through the plates H flow in pipes represented by dotted lines in your diagram?

A. Yes, the overflow pipes.

Q. They go right down to the bottom of the boiler M, do they not?

A. No.

Q. You have shown it so?

A. I will look at the patent drawing. Yes it does.

The Master: When you start this apparatus you have either got to not fire the chamber N and M until after it is filled, or else you fill it before starting with something or other, is that not true?

[fol. 1140] Mr. Wiles: How do they start one of these things off, Mr. Wilson? Tell us that; I do not know.

The Witness: They fill up the still with cold oil. The fire under M and N is your only real primary source of heat. So you have got to start up as if you were charging a batch system.

The Master: You are starting on your initial charge line P; that is in another furnace.

The Witness: It does not go through any other furnace. It simply goes through a system comprised of heat exchangers which you have there. The system gets heat from this (indicating) and from this, which are the only two places directly fired. In starting off you would fill your largest still with oil, and fire the still until vaporization was well under way. It would not be anything like a continuous process for quite a while. After you had got it started, you would start feeding in more oil, then the process would begin operating in the normal way.

Questions by Mr. Royce:

Q. You would start the hot end initially?

A. That would be the way I would start it up.

Q. Do you know if that is the way they started?

A. I don't know that the Barbet process was ever operated. I do not know.

The Master: You charge M with crude petroleum?

The Witness: Charge M with crude petroleum, yes.

The Master: Then, in some way or other, out of M would come the supply of reduced crude that comes down through P?

The Witness: No.

Mr. Wiles: That is fresh crude.

The Witness: That is fresh, cold crude when it came in up here (indicating).

Mr. Wiles: That is a vapor line for a continuous process?

The Master: I see.

The Witness: It is a continuous process that we are dealing with.

The Master: That comes from your supply——

Mr. Royce: There are two sources of crude when you start, one at M and one at the other end of the line P.

The Witness: You do not feed until you get your apparatus fairly well heated up; then you start your feed, and gradually attain steady operating conditions.

Mr. Wiles: All the testimony has been about the normal, continuous operation, not starting up. It has all dealt with what you do after you get started.

By Mr. Rauber:

Q. The plate T5 there, the lowest plate in the series H, has the densest material on it, does it not?

A. Yes. That is the densest of anything that is in the column.

Q. And that goes back to the still M?

A. Yes.

Q. What is your idea as to the composition of that liquid?

A. Well, that contains everything heavier than the heavy kerosene, namely, the light paraffin distillate, heavy paraffin distillate and the heavier constituents of reduced crude.

The Master: What is the reason that vapors do not pass back into the tower there instead of passing through L?

The Witness: They do pass back up the tower; part of the vapors pass back up the tower, a considerable part, in order to——

Q. Most of them, is that not right?

A. Well, I do not know.

Q. If you draw off only a smart part, why, of course the larger part must go up the tower?

A. Yes, if you draw off a small part the larger part must go through the tower. What they do is condense in the liquid on these plates. That is, all the heavier portions condense in that liquid and drive off the lighter ends which come out through 6, 7 and 8.

[fol. 1141] Q. The only lighter ends driven off are those kerosene ends?

A. No, you drive off a little of the paraffin distillate, it says.

Q. Where?

A. In the plates H.

Q. I mean drive them off into the outlets 6, 7 and 8?

A. I rather think he implies that a little paraffin distillate goes out of H, though I do not know that is necessary; but that is the implication.

Q. Those are illuminating or kerosene cuts?

A. I say he states specifically about vaporizing even a little of the paraffin products.

Q. Yes; that is on the plates H.

The Master: 6, 7 and 8 are final cuts. Nothing gets back into the system after it gets to 6, 7 and 8, does it?

The Witness: No, that is correct.

Q. You do not contend they contain paraffin distillate?

Mr. Wiles: Mr. Rauber, where do you think the plates H end? I always supposed these were the plates H (indicating), and these were the plates H' with a big break between them.

Mr. Rauber: That is my idea.

Mr. Wiles: That paraffin distillate gets out of this plate, then goes out to here (indicating)?

Mr. Rauber: The paraffin distillate may go out at that point, but not go any further than the plate H.

Mr. Wiles (continued): Then evaporate in the plates H; that means it is going into the next plate higher.

Mr. Rauber: Where do you find that? I am not sure that you are right.

Mr. Wiles: Mr. Wilson called your attention to it. If I am right, the plates H are this group here (indicating).

The lower plates are called H, and the upper plates are called H'.

The Master: I see that plate is numbered with a T (indicating).

Mr. Wiles: Those are the gauges, I suppose. I think these T's apply to gauges.

The Witness: Yes, temperature indicating devices they are supposed to be.

Mr. Royce: In your testimony you have spoken of T sometimes, haven't you, as referring to the plates there?

The Master: Yes, he has.

[fol. 1142] Mr. Rauber: I believe you have.

The Witness: There may have been a mistake.

Q. I suppose all the illuminating oils vaporize which are evolved on the plates H?

A. Yes.

Mr. Wiles: And then a little of the paraffin distillate, he says.

Mr. Rauber: I do not find that.

Mr. Wiles: Mr. Wilson called it to your attention.

The Witness: It says the methodical boiling on the plates H.

Mr. Rauber: Yes, I see it on page 1691.

Mr. Wiles: Sure.

Mr. Rauber: That would be on the lower plates.

Mr. Wiles: Not unless I am entirely wrong. If you are going to evaporate paraffin distillate on the plates, upper plate H, it is going to get out through the line S-2. It cannot help it.

Mr. Rauber: But if you evaporate it on the lower plates——

Mr. Wiles: It would not be gotten out. It would not be boiled out, as he states it will. It has got to be boiled out and get through the system on the upper plates H'.

Mr. Rauber: I think perhaps we better have the witness give all of that alone, with all due respect, Mr. Wiles.

Mr. Wiles: All right.

Mr. Rauber: We are losing time.

Mr. Wiles: We are certainly losing time fussing around about this kind of a question.

The Master: I rather apprehend that while these are a pair of Siamese twins, that you better confine your examination to the technical witness.

Mr. Rauber: I will try to do so. I think possibly some leeway is permissible.

Q. At any rate, if you are making a kerosene in 8, you would not want any paraffin distillate in it?

A. No, but it clearly states——

Mr. Rauber: That it is an illuminating oil?

A. (continued). That the paraffin distillate—I mean that the heavy illuminants do characteristically contain fifteen percent of heavier oils in this other patent, so I think it is quite possible he did contemplate that some of the paraffin distillate——

The Master: What I want to know, Mr. Rauber: Is this [fol. 1143] all academic or is it applied to some one of these defendants' patents that you are now proceeding to?

Mr. Rauber: To the re-run patent. It is Burton patent No. 1,105,961.

Q. On American kerosene what is the end point?

A. Today or when?

Q. Today.

A. It varies—in good quality household kerosene, between 500 and 575, as a rule, although there are some that are higher than that.

Q. Where is the 90 per cent point?

A. Not very far from that.

Q. About where?

A. Well, it would also be between 500 and 575, as a rule.

Q. Would it be nearer to 500 or 575?

A. If you are talking about the average or——

Q. Yes, the average grade or kerosene.

Mr. Wiles: That question is limited to kerosene of today?

Mr. Rauber: Yes.

The Master: What is the pressure in the Barbet disclosure?

Mr. Rauber: It is stated to be from five to six atmospheres.

A. I think the average 90 per cent point is probably in the neighborhood of 500 or 525 Fahrenheit.

Q. Some of them are considerably lower, aren't they?

A. Oh, yes.

Q. And those that are lower, I suppose, have less of the light ends in them, ends around 375?

A. It may be that or it may be a special, highly refined product for use in incubators, where you want the same height of flame for days upon days. There are many kerosenes made to special specifications. Sometimes they are almost nothing but the heavy ends of the gasoline.

Q. In the general commercial run of kerosene though you would have considerable light ends in a kerosene that ran as high as 600; I mean with ends around 600?

A. Your present-day commercial kerosenes do not generally have end points as high as 600.

Q. Considerably lower?

A. Your end points today are ordinarily between 500 and 575.

Q. And they contain a considerable quantity of material around 375?

A. No, they generally begin to boil around about as 350 or 380.

Q. That is their initial point?

A. Yes, they do not need very much material below 390 or 400.

Q. And you could not meet conditions if you omitted those lighter constituents without also lowering the end point of the kerosene, could you?

A. Yes, you could refine more highly. If you are willing to treat more heavily with acid; you can use a heavier kerosene and get good results. It also depends on the crude, as I pointed out. About all you can say is that for a given treat to meet a given quality standard the more light ends you have in it the more heavy ends you can put in it.

Q. Returning to the Barbet apparatus, would the vapors going from the boiler M to the lowest plate H contain more or less heavy materials than the liquid on the lowest plate H?

A. It would contain less of the heavy constituents than the liquid on the lowest plate H.

The Master: You are still on this Burton patent, are you?

Mr. Rauber: This all has reference to it.

The Master: Just to get down to brass tacks on it, what letter or numeral in Barbet indicates tank 12 of Burton?

The Witness: Are you asking me?

The Master: I am asking you.

The Witness: Now we begin to get into real difficulties, as to finding similarities. They are so entirely different.

The Master: If you do not find similarities, what is the use?

The Witness: Shall I remind you again of what I pointed out the contention of plaintiff's expert on this matter was, in order to indicate any degree of equivalents?

The Master: I was starting in to see if you could read the Barbet disclosure on Burton's patent.

[fol. 1144] The Witness: I certain can not.

The Master: Then that is the end of it.

Q. This cracking chamber P of the Barbet would correspond to the still 3 of Burton, would it not, so far as any cracking is concerned?

A. So far as producing the cracking is concerned.

The Master: You have got to begin at the other end of this system. Burton puts his stuff into still 12, goes through the coil 18 and tank 20, and from there it is run back into 3.

Q. He has got to start at 3 first, does he not, Mr. Wilson?

A. Well, the cracking process takes place in 3. That is antecedent to this Burton operation. At least that is where he gets his pressure tar from.

Q. And that still 3, so far as cracking is concerned, would correspond to the still P of Barbet, would it not?

A. So far as cracking is concerned, yes.

Q. It operates under the same pressure?

A. The same general pressure, yes.

Q. Then the residuum is drawn off from P to the column still Z?

A. Through the line YZ, yes.

Q. And in Burton that is drawn off into the atmospheric pressure still 12, Fig. 2?

A. No, it is drawn off into the tank, first, and then goes to the atmospheric pressure still.

The Master: Is it drawn off into tank 8?

The Witness: No, it is drawn off into the tank, which is the supply tank for this atmospheric still. That is not shown in the drawing.

Q. Indirectly it gets into the still 12, does it not?

A. From that tank it goes into the atmospheric pressure still, in Burton.

Q. In Barbet it goes into the column still at the point Z?

A. If you call it a column still, but, of course——

Q. But it does separate into vapors and residuum liquid, doesn't it?

A. Yes—well, it fractionates material which has already been distilled. That, I would say, is the more accurate description of it.

Q. But the residuum, that is drawn off from still P, and taken up into the column, Z—some of it vaporizes, doesn't it?

A. That mostly vaporizes before it gets into the tower.

Q. I asked you if it vaporizes in Z?

A. I should say it mostly vaporized before it gets into Z.

[fol. 1145] Q. But, anyhow, it changes into vapor and residuum in column Z, doesn't it?

A. No, it probably does not.

Q. Where does it change?

A. Immediately after passing the pressure reducing valve—wherever that may be—that not being shown in Barbet.

Q. In any event, you separate it into vapor and liquid?

A. Yes.

Q. And that is just exactly what you do in Burton's still, 12, don't you?

A. No, you distill in still 12 to which heat is supplied and distill down to coke.

Q. And when you boil off a vapor from liquid, that is distilling?

A. Yes, but when you separate a liquid and a vapor, I don't call that distilling.

Q. You have to form a vapor before you separate it, don't you?

A. Yes, sir.

Q. And that is done in this apparatus, isn't it?

A. Yes, but not in the column.

Q. Where?

A. I said immediately after the pressure reducing valve, which must be in YZ, some place, although it is not shown.

Q. Suppose it is where Z is—as soon as it passes that valve and gets into the still, at Z——

A. That would be a very awkward place to have your pressure reducing valve—up from the ground, when you could just as well put it near the ground.

The Master: Is there any mention of a valve in the position, made in the Barbet disclosure?

The Witness: There is no mention of any valve, there.

The Master: You cannot assume a valve where it is not shown.

Q. Well, after it passes that valve it changes from a vapor to a liquid?

A. Yes.

Q. And that is all that is done in the Burton vapor still, isn't it?

A. No, it isn't all—that is one of the things that is done.

Q. What else is done? You run the liquid down to coke in the Burton still, but that is just simply distilling off the vapors, isn't it?

[fol. 1146] A. No—that is conducting a cracking operation on the material, and destroying the coke forming ingredients.

Q. Does Burton say that he runs down to coke, in his still, 12?

A. I think that is the process he describes.

Q. Is that an essential feature of the Burton process?

A. Not necessarily.

Q. He might not do that?

A. Yes, that is true—he might stop short of running down to coke.

Q. In which case he would be doing just what Barbet is doing—taking off some of his volatile vapors and leaving his liquid?

The Master: What would this mean: "This procedure is carried on until the original contents of the still, 12, have been reduced to a condition of dryness"—what does that mean?

Mr. Rauber: That it is run down to coke, but Mr. Wilson says that is not essential.

The Witness: No, that is not vital.

Q. If you are going to apply that Burton to such a process as the Holmes-Manley process, you will find that they are closer to Barbet than they are to Burton, will you not?

The Master: We are dealing, now, with Barbet.

Q. Is it your position—in other words, if Burton don't run to dryness, he would be doing what Barbet is doing, wouldn't he?

A. No.

Q. He would not be distilling off his vapors and leaving liquid?

A. In Barbet you do separate the vapors, from the liquid, in the column.

Q. But before you can separate them you must form them?

A. Yes.

Q. And that is just exactly what Burton does—forms vapors and separates them out?

A. I think there is a difference between forming vapors by auto-tar distillation—that is, distilling by contained heat, and heating up a body of oil in a still. I am not saying that they may not be functionally the same.

Q. But, to a degree, at least, Barbet distills off—distills off distillate from his residuum, in his cracking still?

A. In that part, yes.

[fol. 1147] Q. And that part is column still Z, plus any part of the pipe past the valve—is that correct?

A. Yes.

The Master: Past the valve?

Mr. Rauber: Yes, sir.

Q. And those vapors go up through that still, that column still—isn't that correct?

A. Well, if they went into the still M, and stayed there, heavy vapors are condensed out.

Q. The vapors do go out there?

A. Some of the vapors, yes.

Q. And they go over into the column still H, don't they?

A. Only the illuminating vapors.

Q. The vapors do get across?

A. Some vapors do get across—the illuminating vapors.

Q. And if any of those vapors did get over and were

not withdrawn through the outlets 6, 7 and 8, or anywhere else, they would again return to the still M, and the cracking chamber P?

A. Unless they were distilled off in still M.

Q. But assuming that they are not?

A. Well, if they went into the still M, and stayed there, then they would go back, shortly, but that involves three very violent assumptions, but if you make those assumptions, that is true.

Mr. Rauber: That is the connection—that you take off your liquid from your cracking still, take off distillate from it, and the question is whether it gets back into your cracking still or not. And that is what I am trying to thresh out with Mr. Wilson.

Q. Then any condensate that goes down into the still M as a liquid and is not boiled off completely, will go through that connecting pipe into the cracking chamber P?

A. Yes, if it is not boiled off in M it will go through into the pressure still P; that is correct.

Q. And all of the vapors heavier than the kerosene cut taken off in 6, 7 and 8 go down through the plates H; is that correct?

A. That is correct.

Q. A small amount of those vapors are drawn through the valve L together with heavy vapors?

A. No, that is not quite right. Of the liquid which passes down into M the lightest portion is vaporized, and that is the material which passes out through L into G.

[fol. 1148] Q. Only a part of it?

A. Why only a part?

Q. Some of it must go up through the plates H, he says, but a small part.

A. Yes, yes.

Q. You agree to that?

A. That is the only way it gets out of the system, yes.

Q. Except through the bottom of the still. It could go out there.

A. But they do not stay as liquid.

Q. Do you mean to say there is a selective separation of the lightest vapors from the mixture, going up from the boiler M?

A. No.

Q. And the taking of those out through the valve L?

A. No. When the liquid strikes M the lightest portions are vaporized and none of the heavier portions are vaporized.

Q. None of the heaviest portions have?

A. Oh, some go along with the lightest portions.

Q. What are those portions? Those heavy portions?

A. Heavy paraffin distillate, primarily.

Q. Heavy lubricating stocks?

A. Heavy lubricating stocks?

Q. A little of everything heavier?

A. Theoretically, a few molecules of even the very heavy ones, yes. But practically all are the lighter materials.

Q. There must be some lighter materials in that boiler M from which they distill?

A. There are lighter materials in the liquid which runs down into the boiler M.

Q. And none in the liquid in the boiler M?

A. Oh, a few molecules.

Q. How many molecules?

A. Well, when we are talking about the number of molecules, as compared with the other molecules present, yes.

Q. Several millions of them present, though, are there not?

A. Yes, certainly. But still that is comparatively few.

Q. How many; how much percentage of the liquid in M would be that light——

A. Of the lightest I should say not a tenth of a percent; of the heavier part of the light paraffin distillate cut which goes out, possibly 10%. You undoubtedly do not vaporize [fol. 1149] all of the light paraffin distillate cut; you simply take off the lightest part of it. All we can say is that the material which is left in M will be heavier than the material which goes out through L.

Q. Now that you have stated about ten per cent of the material in M would be a light paraffin distillate——

A. (Interposing) Would be the heavier part of light paraffin distillate. Note me as correcting, please.

Q. What is the percentage of vapors under those conditions?

A. That were the heavier part of light paraffin distillate?

Q. What percentage of the vapors going up would be the same light paraffin distillate?

A. Well, not having defined accurately what we mean by light paraffin distillate, or the operating conditions, it is quite a guess; but I should say that if you had ten per cent of the heavier part of light paraffin distillate in the still M, probably 50 or 60 per cent of your vapors would be the heavier part of light paraffin distillate.

Q. What were the other forty or fifty per cent?

A. Most of it would be still lighter products the lighter end of light paraffin distillate. A little bit of it, maybe five per cent, would be products even heavier than light paraffin distillate.

The Master: Mr. Wilson, I am still groping around. I want to find out this: Can we assume that the tank M of Barbet corresponds to the tank 3 in Figure 1 of the Burton patent No. 1,105,961?

The Witness: No, it does not at all.

The Master: Why not?

The Witness: Because Barbet is distilling in his tank M light paraffin distillate out of reduced crude oil; Burton is distilling the light ends of pressure tar out of pressure tar. The operations are in no way comparable.

Mr. Rauber: I do not believe the Master——

The Witness: Oh, excuse me. This tank 3, the pressure still. No, there is no comparison at all. Tank 3 is the pressure still of Burton.

Q. And tank P is the pressure still of Barbet, is it not?

A. Yes. I thought the Master was referring to the other.

Q. So tank P would compare with tank 3 of Burton as to pressure?

A. That is correct.

The Master: Then what in Barbet corresponds, starting [fol. 1150] with tank P? You say that P is the equivalent of tank 3 in Burton.

The Witness: Yes.

The Master: Where is the coil 5 out of P?

The Witness: There is not any such coil shown. The vapors pass through the line Z into the base of the tower, and pass up through the tower, or for the most part pass up through the tower, and the heavier part is condensed, and go out through X. The lighter parts pass up and are mainly condensed in R.

Q. And go over and condense in the other parts of the column not shown?

A. The illuminating vapors pass over into the other column.

Q. And any other lighter vapors go up to a column not shown?

A. Yes.

Q. For gasoline?

A. Yes.

The Master: Then we will say the train through the line 2 into the tower Q is the line 5.

The Witness: Of course, in Burton, the condensation is under pressure, whereas this is all under atmospheric pressure. That is one of the many differences.

The Master: And the product runs off as through *u* and *u'*, and where is the condensate?

The Witness: Possibly I can give it to you in more detail.

The Master: Where does the condensate of this tower go?

The Witness: Part of it is drawn out here and part of it through here (indicating). Part of it is drawn out through X and part of it through——

The Master: Where does that paraffin distillate go—that, is, the final product?

The Witness: That is the final product, yes.

The Master: And that corresponds to the product in 8 of the Burton patent?

The Witness: No, the product in 8 of the Burton patent is gasoline and this is paraffin distillate.

Q. To get into the tank corresponding to tank 8 of Burton you have to go through the other towers, do you not—to get your light ends?

A. Oh, your light ends are condensed—I will just run [fol. 1151] over that. The paraffin distillate goes out through 10, the illuminating vapors pass over into H and are segregated there into 6, 7 and 8, and the lightest vapors which mostly nearly resemble those of which Burton's product entirely consisted, pass out through the top of the tower.

Q. Those towers are to accomplish a stripping such as you have described in connection with the Humphreys' re-run patent?

A. No, they are to separate the product. This main proposition here is simply a method of running crude continuously and getting the products off continuously.

Q. And cracking continuously?

A. That is a minor part of his operation.

The Master: I have tried to follow the steps of Burton in this Barbet patent. I have not made such progress yet.

Mr. Wiles: I tried very hard when plaintiff's expert was telling us about it, and did not succeed.

Q. There are light vapors produced in that cracking chamber P, are there not?

A. There are certain illuminating vapors produced.

Q. The liquid residuum from Y is carried into that column Z?

A. Yes.

Q. And on its way to the column Z and possibly in column Z it is changed into vapor and liquid?

A. Yes.

Q. The liquid flows down to the bottom of the column?

A. Yes, except that the lighter parts of the liquid may be stripped out of it.

Q. Yes, and join the vapors going up through the column Q'?

A. Yes.

Q. The vapors from that column Q' go up in the rising pipe to R, do they not?

A. Yes.

Q. And some of them condensed out there?

A. Yes, the paraffin distillate is condensed out there.

Q. The light paraffin distillate?

A. I should assume both the light and possibly some of the heavy.

Q. What is the temperature of that condenser R?

A. I do not know that he mentions an exact temperature, but a temperature sufficient to condense the paraffin distillate and allow the illuminating vapors to pass on.

Q. He gives the temperature, does he not, as 70 degrees Fahrenheit?

A. I do not think so.

Q. Or 70 degrees centigrade.

A. In R are you talking about?

Q. Yes.

A. I do not think so.

Q. Oh, it is in V that it is given, I believe.

A. Well, that is quite a different matter, of course.

Q. He states that R is fed with hot water, does he not?

A. Whereabouts?

Q. At page 1692, next to the bottom paragraph.

A. Well, it says, "care will be taken not to send cold water into the condenser R; it can be fed with hot water coming from B."

Q. "If hot water is lacking the desired heating will be produced by a little direct injection of steam at the bottom of R"?

A. Yes.

Q. When you chill out—the purpose of that is to prevent chilling and clogging of the pipes by paraffin, is it not?

[fol. 1152] A. Yes.

Q. And when you chill out paraffin from a light paraffin distillate you use temperatures below freezing, do you not?

A. Well, as I said, paraffin chills out at ordinary temperatures. If you want to get all the paraffin out you must chill to law temperatures, but if you want to prevent the clogging of the condenser you have got to prevent any paaffin from separating out, because it will separate on the walls and gradually built up. There is no comparison between the temperatures that are used in separating out all of the wax and the temperatures which will separate out some of the wax.

Q. And of course, any gas oil there would not separate out and clog the pipes, would it?

A. No, certainly not. It is only wax that separates out.

Q. Of the residual liquid coming from that cracking chamber P, and going up into the fractionating chamber Z, a portion of the lighter materials—

A. Excuse me. The fractionating column is Q and Q'. Z is the pipe.

Q. Q and Q'; a portion of the liquid vaporizes; that is correct, is it?

A. Yes.

Q. The remaining portion goes down to the bottom of the column?

A. Starts down at least.

Q. And a portion of it reaches the bottom of the column?

A. Yes.

Q. And is withdrawn through the pipe X?

A. Yes.

Q. The rest of the vapor, or a considerable portion of them, pass upward through the column Q and Q', do they not?

A. Yes.

Q. Then, all of the vapors that reach the top go into the cooler R?

A. Yes.

Q. And the vapors not condensed there go through that pipe you have marked "illuminating vapors" over to the column H, H'?

A. Yes. Of course, the patent states what those vapors are.

Q. Those vapors would carry some gas oil vapors, would they not?

A. What do you mean by "gas oil vapors?"

Q. Vapors of materials that would sometimes be placed in gas oil?

A. By Barbet or in the present practice today?

Q. In the present practice today.

A. Undoubtedly there are some of the materials which we today would put in gas oil which go over as part of those illuminating vapors; yet, that is correct.

Q. And of course, the quantity that went over there would depend upon how drastic the cooling in the cooler R was?

A. Yes, but there is a definite disclosure as to how drastic it is to be.

Q. That is, it must be cooled with warm water, hot water?

A. Well, of course, even warm water would condense the kerosene, as far as that is concerned, so that is not what limits it.

Q. But it must be hot water and the purpose of that is so that no wax will chill out?

A. And separate on the cooling surfaces, yes.

The Master: Those must go over as vapors not as liquid, is that right?

The Witness: The illuminating portions go over as vapors, yes.

Q. And also, whatever heavier vapors may go over with the illuminating vapors go over as vapors, too, do they not?

A. Well, I don't think anything heavier than the illuminating vapors goes over.

[fol. 1153] Q. You said that some vapors that ordinarily would go in gas oil go over?

A. Yes, but those are part of Barbet's illuminating vapors.

Q. They form a part of what you term "illuminating vapors"?

A. He takes it out at 6, 7 and 8.

Q. That is all part of Barbet's disclosure, is it not?

A. It is a splitting of his illuminating vapors, after they are condensed to liquid.

Q. Which in this country we call kerosene; did at that time?

A. Yes.

Q. Now then, any vapors that do not go into that lower pipe S'' or the intermediate pipe S'—

The Master: Wait until I get the pipes S' and S''.

The Witness: Here (indicating).

The Master: Oh, yes.

Q. (continued). And eventually go into the kerosene draw-off pipes 6, 7 and 8, go down through that column H, do they not?

A. If there is any such, which I see not the slightest indication of.

Q. But you will admit that there are some gas oil vapors going over through that pipe, that you have marked "illuminating vapors"?

A. Why, I have indicated very clearly exactly what my position was; that there was no gas oil cut in Barbet; that there is certain material which we might today put into gas oil but which Barbet considered part of his illuminating vapors and withdrew as his heavy illuminants.

Q. Your position is that Barbet makes no gas oil cut?

A. Yes.

Q. He withdraws no gas oil cut from his system?

A. No.

Q. And it all stays in the system, unless it is cracked?

A. No gas oil cut is taken out.

Q. We agree on that; that no gas oil cut is taken out of that system?

A. Yes. There is no gas oil cut in that method of operation.

Q. Now, is this re-run process of Burton used in the Cross process?

A. Certainly not in a narrow sense.

[fol. 1154] Q. Only in a broad sense?

A. Only in a very broad sense is it used.

Q. What is the distinction between the narrow and the broad sense?

A. Well, the narrow sense is in conducting a pressure distillation at four to five atmospheres pressures—

Q. Does Cross use four to five—that is the narrow sense?

A. Yes.

Q. Pardon me.

A. (continued). Four to five atmospheres pressure, taking the tar out and redistilling it at atmospheric pressure to separate out the coke-forming constituents and re-cracking under similar conditions—the distillate thus produced, which is a clean cracking stock.

Q. In that sense Cross does not use the Burton re-run invention, in that narrow sense?

A. In that narrow sense Cross did not use the Burton re-run invention.

Q. Is the invention of the Burton re-run patent, No. 1,605,961 used in the tube and tank process?

A. No, I should not consider that it was.

Q. Not in any sense?

A. The two types of processes are so different in their method by which they produce cracking and the character of the stock that really I should not consider them to be carrying out the Burton re-run process, and the same is true of the Cross.

Q. Now, I will call your attention to the Clark patent No. 1,119,496, which is petitioner's exhibit 177. What do you consider to be the essential features of this invention?

A. The maintenance of a large body of oil at a cracking temperature in an unfired receptacle by circulating rapidly to and from that receptacle a rapidly moving stream of oil, [fol. 1155] which is exposed to direct heat, with provision for coke deposition in the large drum and away from the heating surfaces.

Q. Do you consider that this invention is used in the Holmes-Manley process?

A. Certainly not in the narrow sense in which it is described by Clark. There is a possibility under a broader concept, but as the invention is described by Clark I should say it was not used in the Holmes-Manley.

Q. What is that broader concept in which it might be used; can you give us an idea of that?

A. The broader concept, which is involved in this, is of course the fact that the cracking is mostly done in a different place from where the heat is applied. In the Holmes-Manley the cracking and coke deposition is mostly done in a different place from where at least the bulk of the heat is applied, but there is no circulation from and to the bulk supply as Clark specifically discloses.

Q. There is some heating done in the Holmes-Manley on the bulk supply, is there not?

A. Yes.

Q. In connection with this Exhibit 411, this represents the Holmes-Manley process as used at Whiting, is that correct?

A. Yes.

Q. Is that the same in all of the details shown here as the Texas Company plant at Tulsa?

A. The temperatures and pressures at the Tulsa plant at the time I was there at least were not the same as those that are shown here. The general arrangement of the system is essentially the same, though at least part of the stills at Tulsa have tar heat exchangers on hem such as are shown on the model, but which are not of the Whiting stills.

Q. That is not an essential feature by any means, is it; it is an unimportant difference?

A. Well, it just depends upon what you mean by that.

Q. It merely results in a slight economy, I suppose, does it?

A. Yes, there are two different ways in which you can make use of the heat in the tar, one is by putting it through heat exchangers; the other is by flashing it and letting it distill itself.

Q. And in the Tulsa, Oklahoma plant it flashes, does it?

A. No, not in the Tulsa, Oklahoma plant. In the Tulsa [fol. 1156] plant at least some of the stills when I was there, had the tar heat exchangers which we do not have.

Q. That is not shown on this drawing?

A. No.

Q. In all other respects this would represent the plant at Tulsa?

A. Well, no; I certainly would not say hat. I think that the features which are shown here are all present in the Tulsa operation, save for some specific conditions, but there are many things at both Whiting and Tulsa that do not show on this drawing, of course.

Q. What specific conditions are changed in the Tulsa plant from those indicated in this exhibit?

A. Why, the stills which I saw operating were operating at somewhat lower pressure.

Q. What were their pressures?

A. As I recall it, the pressure was about two hundred and fifty pounds in some of the stills, and I think their pre-heater outlet temperatures were ten degrees or so lower than those shown here.

Q. I do not see the pre-heater outlet temperature shown on this Exhibit 411.

A. It is right there.

Q. What is it?

A. 800 degrees Fahrenheit.

Q. That is from the heating coil?

A. Certainly.

Q. Then there is another coil above that?

A. That is what we call the economizer coil.

Q. That is in its nature a pre-heating coil, too, isn't it? They are both part of one pre-heating coil?

A. No, they are separate pre-heating coils, with quite a distance between them. They have different gases going up through them, but heat is picked up in each, of course. Different oil goes through the coils and different gases go up around the coils.

Q. What is the temperature of the gases around those stills—the furnace gases?

A. Well, that is a pretty hard thing to say, as you know, on a system of that kind. The temperatures will vary depending on whether you are near the flame or far above it, or whether you are near the stills, or in the lower part or the upper part.

[fol. 1157] Q. How does the consumption of gas in that furnace compare with that in the pre-heating coil furnace?

A. Something like a fifth as much gas is burned in there as is burned in the pre-heating coil. That is something like one-fifth as much fuel, based on heat value. We use gas around the stills and we generally use oil to heat the coil.

Q. What is the temperature of the oil in those stills?

A. It drops from the first to the last still. It averages, with 800-degree inlet, around 750 or 760, sometimes 770.

Q. This is the feature that is shown in Adams, this idea of heating the oil in one place and then letting it stand in bulk in another to crack?

A. Yes, that feature is, I believe, first disclosed in Adams.

Q. The feature of heating oil in one place and letting it distill in another is old in the art, isn't it?

A. Well, of course, the feature that we are talking about it a feature applied to a pressure-cracking process for the making of gasoline. I am quite sure that that feature is not old in the art, at least so far as I know.

Q. What would be the difference in cracking for gasoline or for anything else, so far as that feature is concerned?

A. Well, a great big difference is, of course, that in doing just ordinary light cracking to make kerosene you do not have the coke deposition trouble to anything like the extent you have in a gasoline making process.

Q. You do make some kerosene in this process, do you not?

A. As a final product?

Q. No, from your stills; you get some gasoline vapors?

A. Some kerosene vapors?

Q. Kerosene vapors.

A. Do you mean coming out of the system?

Q. Out of the stills.

A. Out of the stills themselves, certainly, yes. If we were content to let it go out and not keep putting it back, it would be very much simpler, of course.

Q. And if you were cracking to make kerosene you would make some gasoline, too, would you not?

A. Yes.

Q. I call your attention to the patent to Schuchow, et al., Exhibit 129. Oil is forced in through a pipe C, is it not, under pressure?

A. Yes, some kind of oil is.

Q. Then it passes up through the coils *d*?

[fol. 1158] A. Yes.

Q. And it is heated in those coils under super-atmospheric pressure, is it not?

A. Yes.

Q. Then it goes into the chamber A?

A. Yes.

Q. And may be either returned through the pump B or withdrawn?

A. Into the pipe D?

Q. The pipe D, and then either through the pump B to the pipe C or passed out of the system?

A. Yes.

The Master: Are you referring to page 1621?

Mr. Rauber: That is correct, yes.

The Master: I do not get the letters. Where is C?

The Witness: Down at the bottom, your Honor; the lowest pipe in the drawing (indicating).

The Master: You said pump C.

Mr. Rauber: The pump B.

The Master: And the pipe C.

Q. In passing upward through the coils *d* would there be any carbon formed at all, heated under pressure?

A. There might or might not.

Q. How?

A. There might or might not.

Q. Do you think you could heat oil under pressure and not form carbon?

A. I am quite sure I can.

Q. And crack it?

A. Not and crack it, no.

Q. What oil could you pump through a long coil like that under presure and not crack, heating at the same time?

A. Any oil.

Q. Heating it to a distilling temperature under pressure?

A. Yes, certainly.

Q. What pressure?

A. Any pressure.

Q. 80 pounds.

A. Certainly.

Q. How long would your coil be?

A. The points are not at all relevant; it is all a question of what temperature you heat it to.

Q. You would heat it to the distilling temperature suffi- [fol. 1159] cient to distill, would you not, a considerable quantity? That is assumed.

A. Again, you can do that very readily without getting any appreciable amount of carbon in any oil except an oil

that contains a lot of dirt and salt or something of that kind.

Q. You would not get any carbon in the coils *d* under those circumstances?

A. I say you might or might not. You could heat it without getting carbon, and it might be that when you heated it you would get carbon, depending on the temperature you heated it to.

Q. What temperature would you have to heat it to in order to vaporize it?

A. That is not a definite question at all. It depends on how much of it you want to vaporize and what the oil is. There is no definite answer you can give to that question.

The Master: You mean it has to be relative, to your pressure?

The Witness: Relative to your pressure and relative to your oil, and relative to how much of it you want to vaporize. You can vaporize a little of it at very low temperature.

Q. Under pressure?

A. Certainly.

Q. If you let it remain in the chamber A for a considerable length of time at that temperature it would not crack?

A. What temperature?

Q. A temperature sufficient to vaporize it?

A. No, especially not when it is cooled by incoming liquid. But even apart from that, you can do a lot of vaporization without cracking.

Q. Under pressure?

A. Certainly.

Q. You would distill under pressure, any of four or five atmospheres, without cracking?

A. It depends on what you distill.

Q. Distill a heavy residuum or anything like that.

A. Distill a heavy residuum?

Q. Yes.

A. No, you could not; not without cracking.

Q. If you distilled anything heavier than kerosene, or gasoline, under pressure of say four or five atmospheres, how could you do it without cracking?

A. Well, that would depend on the amount of light ends [Vol. 1160] you might have present. If you have a lot of light ends present, or quite a bit of dissolved gas, and, particularly, if you have crude oil with water in it, you could distill off very much heavier ends than gas oil, even under pressure, without getting cracking, except, possibly, getting a localized coke deposit on the heating surfaces.

Q. If you have enough water in it to reduce the effect of the pressure, you mean?

A. Yes, sir, or enough light ends of the oils, itself.

Q. But if it had no water, or light ends, then it would crack?

A. If you have an oil which contained nothing lighter than gas oil, it certainly would have to crack, before you could distill it under that pressure, in the absence of steam.

Mr. Wiles: You are still talking about the 80 pounds that you mentioned originally?

Mr. Rauber: Yes, or anything heavier.

The Witness: The same would be true for higher pressures.

Q. But not under slight pressures?

A. Well, the lower the pressure the heavier materials you could distill withou cracking.

Q. I notice that in this patent, on page 1622, of the Book of Exhibits, in the eighth line from the bottom of the page, it says: "The material may be subjected to a process of decomposition." Can you decompose an oil without cracking it?

A. Certainly, not completely, or anything approaching completely. I suppose you might have certain changes take place, which would be called decomposition, but still would not meet the definition of cracking, but if it is all decomposed it is cracked.

Q. If it is decomposed in the ordinary sense, it would be cracked?

A. Yes, I think the ordinary sense of decomposition means cracking.

Q. Then, in this Schuchow process, heating to decomposition, he would supply most of his heat in the coils, *d*?

A. Yes.

Q. And very little in the receiving tank, or separator—cylinder, A?

A. Yes, I should say a negligible amount of heat would be supplied to cylinder A.

[fol. 1161] Q. Then, in its broadest sense, this patent heats the oil and conveys it into a large still, in substantially the same manner as he Holmes-Manley does?

A. No, far from it.

Q. Or, in the sense of the Clark patent, as applied to the Holmes-Manley?

A. No. The whole essence of the Clark process is holding a large body of oil at a cracking temperature, and conducting the cracking away from the heating surfaces. You have here a small body of oil in a separating chamber, cooled both by radiation and by liquid coming into it. ——

Q. Where does the liquid come into that? As I see it, the feed comes in through the pipe R—isn't that correct?

A. The feed comes in—I am just looking to see where that is.

The Master: On page 1623, it says: "Independent of the flow of the liquid which is being distilled into the pump through he cock R, part of the liquid which is being distilled can be admitted directly into the cylinder A, for the dephlegmation of vapors formed in the oils."

The Witness: Yes, that is correct.

Q. How would dephlegmation be accomplished by a liquid entering Chamber A?

The Master: There is a curious translation there—"Independent of the flow of the liquid which is being distilled, into the pump"—that should be, in my judgment, a comma, there, and then it should read, "Part of the liquid which is being distilled can be admitted through the cock R, directly into cylinder A."

The Witness: No, I don't think so. The cock R, you see, is down on the bottom—that is what he refers to in the first part of the sentence. He put part of his feed through R, and another part directly into A.

The Master: Oh, I see.

Q. If he put a liquid in to dephlegmate, or condense any of the vapors into cylinder A, it would enter the top, would it not?

A. I presume so.

Q. That is the usual place for it to enter, isn't it?

A. It would be, if you are going to get the best effect of dephlegmation—that is where it would enter. If he puts it into A, I think he would put it in some place——

The Master: Not what he should, could, or would——

[fol. 1162] The Witness: He doesn't show where he puts it in.

The Master: Suppose he puts it in through D?

The Witness: No, that is going the other way. That is the overflow that is coming down.

Q. In ordinary chemical engineering you would put it at the top of the stillhead, would you not, if you wanted to dephlegmate?

A. Yes.

Q. That would be that little cylinder on top of cylinder A?

A. I don't know what is in that cylinder on top of A.

The Master: The little cylinder is collecting the vapors and passing them over.

Q. And that is where you would have the dephlegmation, if you dephlegmated, at all?

A. There is this to be watched, however—if you put the feed liquid in there at the top, then you are likely to contaminate your feed with the dirty petroleum residuum that you are working in.

Q. Contaminate the feed?

A. I should have said contaminate the distillate. Therefore, I think you would not put it into the upper cylinder, but you would probably put it in some place in the cylinder A, itself, near the top.

Q. What harm would contamination of those vapors do?

A. Well, if you are trying to run particularly a fractional distillation, certainly the whole object of that is to separate out the tar from the relatively clean light-colored vapors.

Q. If you are running it for decomposition or cracking, why, he would not care so much?

A. Of course, when he is running for decomposition or cracking, he is planning to produce mainly gas and he probably does not want a lot of tar go over with the gas.

Q. Leaving that aside for the moment, and assuming he might want to form some liquid.

A. In that case he would not want it contaminated.

Mr. Wiles: Why assume something he does not disclose?

Mr. Rauber: He discloses decomposition of oil.

Mr. Wiles: He discloses fractionation and gas-making and nothing else.

Mr. Royce: In asking this witness now he can assume anything he wants.

Mr. Rauber: Yes. I think one witness is enough at a time.

The Master: Yes, go on with your question.

[fol. 1163] Q. If he were cracking for decomposition or to get some liquid and wanted to dephlegmate, he would get it at that part of the still——

A. No.

Q. If he wants to get it into the chamber A——

A. I think it would be a very foolish thing, but I will not say he wouldn't do that. If he did that he would have the best chance in the world to get some of the tar into his liquid, which he is working for and wants to keep clean.

Q. Do you know whether any of the Burton stills did ever feed liquid down through the vapor pipe?

A. We do today, but——

Q. Yes.

A. (continued). It is clean liquid. It is not a petroleum residuum by any means.

Q. If this man wants to dephlegmate without contaminating his vapors, he would use a clean liquid?

The Master: That would depend upon what he says in his patent.

Mr. Rauber: He says he does not dephlegmate by means of his feed.

The Witness: By part of his feed, yes. His feed is clearly shown to be nothing else than ordinary residuum.

Q. Leaving aside for the moment the question of contamination: A chemical engineer would naturally put his dephlegmating liquid at the top of the stillhead, wouldn't he, near the top? He wouldn't put it down——

A. He wouldn't be a good chemical engineer if he would leave aside the subject of entrainment.

Q. You do not ordinarily put it into the body of the still?

A. This is not a still. This is simply a separating chamber.

Q. He wouldn't put it even at the bottom of the dephlegmator?

A. He would put it in the upper part of the separating chamber, but far enough below where the vapors are leaving to prevent contamination.

Q. Then, as that liquid came down he would cool some of his vapors, wouldn't he?

A. Yes.

Q. And strip out of them the heavy ends?

A. No, strip out the light ends.

[fol. 1164] Q. He would condense the heavy ends and strip out the light ends?

A. Condense the heavy ends of what?

Q. Of the vapors.

A. What vapors?

Q. Any vapors that he is dephlegmating.

A. Yes.

Q. And let the light ends go on?

A. And let the light ends go out the top, yes.

Q. Let the light ends go out the top, through the pressure release valve?

A. Yes.

Q. And he would in that way return the heavy ends to the distillation process or cracking process?

A. No, simply return it to the separating drum.

Q. Yes. And from there it would be carried by the feed down through the pump and up through the the coils again, wouldn't it?

A. Yes, it would pass through the coils again; that is true.

Q. His stripping action accomplishes the results that you mentioned in regard to the Humphreys re-run patent, does it not? That is, it condenses all the heavy ends and strips out of them the light ends? Just answer that yes or no.

A. Well, as between yes and no, I think no is the more accurate answer, though neither one is strictly accurate.

The Master: I would like to know one thing: Is dephlegmation of vapors and stripping of the vapors the same thing?

The Witness: No.

Q. What is the difference?

A. Dephlegmation is ordinarily defined as the act of [fol. 1165] partial condensation. There stated, is some confusion, as you may recall previously in the chemical engineering literature, but it is ordinarily applied to the step of fractional condensation. Then, if these dephlegmated liquids or liquids which are condensed by the dephlegmating apparatus or process are returned countercurrent to the stream of hot vapors or steam, you will get a stripping action which would tend to remove the light ends from these vapors which have been subjected to partial condensation or dephlegmation.

Q. That is not the usual type of dephlegmator?

A. Well, our common use of the term is loose,—it varies. As we use the term we may have a column surmounted by a dephlegmator, the dephlegmator being really the partial condenser.

Q. The whole thing might be called the dephlegmator?

A. It sometimes is; but not ordinarily.

Q. But the very meaning of the word means to take out and strip out the heavy ends?

A. Yes, which is accomplished by the partial condenser and is not accomplished by the stripping part of the column.

Q. It is accomplished most effectively in a bubble tower or fractionating tower, is it not?

Mr. Wiles: What do you mean by "it"?

Mr. Rauber: Dephlegmation.

A. Removing of the phlegm?

Q. Yes.

A. The thing that makes that certain is the action of the partial condenser at the top of the column. Then, as the condensate runs down it is subjected to the stripping action.

Q. The liquid goes down——

The Master: Now, I want to see what is stripped. What is the object that you strip off?

The Witness: Strip out the light ends from the liquid going down.

The Master: In one case you dephlegmate the heavy particles?

The Witness: That is right.

The Master: In the other you strip out the lighter ends?

The Witness: Yes, from the liquid which has been dephlegmated.

Q. The result in both cases is the same, is it not, the separation of the phlegm or heavy ends from the light vapors?

[fol. 1166] A. Well, unless you keep both things in mind you can make lots of mistakes in designing a fractionating tower. You have got to do one thing efficiently first; that is, to prevent all the heavy vapors leaving the device.

Q. That is done by your cooler, is it not?

A. That is done by your cooler and the upper part of your fractionating column.

Q. Yes.

A. (continued). In order to accomplish a condensation which won't allow any heavy ends to go by, you must necessarily condense a certain amount of the light ends, that you eventually want to take out of your system as vapor, and you return that mixture through the stripping section in order to strip out these light ends and send them up to the top of the tower again, where they make another effort to get out.

Q. That is the most effective type of dephlegmator, is it not?

A. Well, as I say, we do not ordinarily refer to that as a dephlegmator.

Q. It is commonly referred to as a dephlegmator in the arts, is it not?

A. The whole thing?

Q. Yes, that stripping device?

The Master: Do you call Humphreys' tubes; dephlegmators?

The Witness: The upper part of Humphreys' tubes is the dephlegmator.

Then, as it returns——

Q. Are you speaking of the patent?

A. Speaking of the patent, or of the tube with the radiator in the top. In that case the radiator is what we would

call the dephlegmating part of the apparatus. Then, as the liquid returns down the elongated conduit it passes through the stripping section where we strip cut the light ends from the condensate.

The Master: Dephlegmation is produced at the top end, stripping at the intermediate and lower ends?

The Witness: Yes, that would be it. That is the ordinary distinction which we use.

Q. That is just used in your refinery?

A. No.

Q. Not in the alcohol——

[fol. 1167] The Master: That is what Humphreys used, is it not?

The Witness: Well, I am not sure.

Q. In alcohol stills are not the stripping actions that you have described and dephlegmation just the same? Does not dephlegmation cover both terms?

A. I am really not familiar with the nomenclature of the alcohol industry. I am not in that industry.

Q. Is that true of the benzol industry?

A. When you come to talking about the terms that are used in the industry, you cannot tell from text books. Now, I have seen various things in the text books and I really could not answer as to what they use in those industries.

Q. Of course, they might miscall anything in a plant?

A. Certainly. Both usages of the term are made.

Q. But you could not have a dephlegmator designed to accomplish its purpose of removing phlegm from light vapors without getting a stripping action?

A. You certainly can, yes.

Q. And to separate all of the light vapors from the heavy ones, from the phlegm?

A. You would separate the phlegm and get out the phlegm.

Q. With some light vapors?

A. With some light vapors.

Q. Unless you strip them——

A. And to get rid of the light vapors, then you must strip; but you can get rid of the phlegm of your product, which is the strict definition of dephlegmation, without stripping.

Q. You do not get rid of your phlegm unless you condense all your light vapors, do you?

A. Yes, in a good dephlegmator.

Q. Every bit of it?

A. What do you mean by "phlegm"?

Q. All of the heavy ends.

A. Oh, again, if you are talking about a few molecules—relatively speaking, there might be a few molecules go through, even if you had an almost perfect dephlegmating device.

Q. The usual type of dephlegmator, though, includes that step of stripping, is that not true, as used in the text books?

A. No, it does not. For example, our dephlegmators in almost all our Holmes-Manley stills do nothing else but condense. There is no other action takes place in them. They are up above fractionating towers in some of the designs. [fol. 1168] Q. In any of the literature did you ever see stripping used as a term together with dephlegmator?

A. I do not have any idea as to just where it is described. I cannot point you to a page, no.

Q. You have never seen it?

A. I think I have, yes.

Q. But you have often seen stripping as a part of a dephlegmator tower?

A. I never—I may have seen the two terms used together, I do not know. I do not recall now any particular. The stripping action is very frequently overlooked by designers of towers.

Q. Not in alcohol stills, is it?

A. Well, they often do not separate the two functions which have to be performed, just as you indicate.

Q. They do it all in one still, one dephlegmator?

A. They do it all in one tower.

Q. That is where the term came from, is it not, from the alcohol art?

A. I have seen the term dephlegmator applied to the partial condenser at the top of the tower only; I have also seen it applied to the whole tower as well.

Q. I call your attention to the Kittle patent, French patent No. 417,532, forming petitioner's exhibit No. 131. In this patent you supply oil to a tank A, do you not?

A. Yes, A.

Q. At 100 pounds pressure?

A. 95 pounds, I think it figures. Approximately 100 pounds.

The Master: Where is A?

The Witness: A is the large bulb in the upper righthand corner of Figure 1.

The Master: Are those symbols used in the French drawings to indicate some particular kind of tank, or do they indicate that this was actually some kind of a spherical——

The Witness: Well, that seems to be what he indicates, certainly, and of course that is a form well designed——

The Master: I think if it is symbolic it means just any kind of tank; wouldn't it?

The Witness: I do not think you would be departing from the disclosure seriously if you put in another kind of tank. I think that probably for use on automobiles, which is what he had in mind——

[fol. 1169] The Master: But it would indicate a closed tank?

The Witness: Oh, yes.

Q. Do you recall what kind of oil is supplied to the chamber A?

A. Why, it says right in the first few lines, "The vaporization of oil, of oil residues, crude oils, tar oils, and other volatile materials." In other words, it would include most anything.

Q. And further on he specifies, on page 1645, a gas oil or solar oil. That is near the bottom of the page, immediately above those columns of data.

A. Yes.

Q. What is solar oil?

A. Well, that is a term that has almost gone out of use. It has never to my knowledge, been used in our refinery since I have been there. I know it is in the literature quite often, but I really don't know. I probably should, but I really do not know just what solar oil is.

Q. It is a light gas oil, isn't it? Do you agree with this description in the book I show you (handing witness)?

A. Yes, I think that is probably correct.

Q. And that would be a light gas oil?

A. Yes.

The Master: Or heavy kerosene?

The Witness: Yes, an old-time kerosene.

The Master: And "solar" would indicate something illuminating.

The Witness: I think that probably referred or may have referred to the process of sunning the oils. I do not know whether it did or not. That was a method of bleaching which was used in earlier times.

Q. That would boil upwards of 500 degrees Fahrenheit?

A. Yes.

Q. Substantially the same oil that Burton used?

A. Very much, yes.

Q. That oil was then forced down through a pipe marked D?

A. Yes, a coiled pipe in a lead bath.

Q. Have you any idea how that pipe would be?

A. Well, it could be most anything, of course. The disclosure is given.

Q. The temperature of the bath is given as 700 degrees centigrade, is it not?

[fol. 1170] A. Just where is that?

Q. On page 1645.

A. Yes.

Q. The temperature of the oil would be somewhat less, would it not?

A. Yes. He says ordinarily it would be about 500 centigrade. That is 932 Fahrenheit.

Q. Then it would go into the chamber F?

A. Well, I don't know whether that is the oil temperature as it goes into F or whether that is the temperature as it is leaving.

Q. That would be the maximum temperature that you could get, or would get?

A. No, you could get higher temperatures than that.

Q. If you run slow enough?

A. Yes.

Q. But it has a temperature lower than that in certain parts of the tube, does it not?

A. Yes, and it might be higher than that at other parts. I do not know where that temperature of 500 refers to, as I say, whether it is the outlet of the coil or the outlet of the separating chamber F.

Q. That would be a little above the temperature necessary to crack oil, would it not?

A. Yes, that is certainly in the range generally used in cracking processes,—that or higher.

Q. There is much less heat transferred into the oil per cubic centimeter of oil in the chamber F than in the tubes D, is there not?

A. I do not quite get what you mean by per cubic centimeter of oil in the chamber F. Do you mean per cubic centimeter of the oil which passes through the system?

Q. No, oil container in the chamber F.

The Master: Would that oil accumulate heat and be hotter in the chamber F than it was in the tube?

The Witness: Such oil as might be in the chamber F would be hotter than in the tube, because it is surrounded by a lead bath at a higher temperature than the oil.

Q. A process of cracking uses up considerable heat, does it not? In other words, it is an endothermic reaction?

A. It does absorb a certain amount of heat. The great problem, however, is to heat it to a cracking temperature.

[fol. 1171] Q. And you don't know just how much it does absorb in cracking?

A. No, I do not.

Q. Or in vaporizing?

A. I can tell pretty well how much it absorbs in vaporizing at atmospheric pressure, but in high temperatures and pressures, we don't have very accurate data. I have a rough idea but I cannot give you very accurate figures.

Q. The rate of heat transfer into a body of liquid, or gas, is greater, the greater the area of the transferring surfaces, to the quantity of oil being heated—is that not true?

A. It depends on the velocity of the oil—the greater the velocity, the greater the transfer, and the greater the temperature difference the greater the transfer and the greater the area the greater the transfer.

Q. So that in the chamber F there is less temperature gradiant, and less velocity and less surface for the quantity of oil contained, than there is in the tube D—isn't that true?

A. I don't see how you could base it on the quantity of oil contained.

Q. I mean, the volume of the chamber F.

A. You mean the volume of the chamber?

Q. Yes.

A. Yes, you would get less heat transfer per unit volume of the chamber in F, than you would in the coil.

Q. Have you any idea how much less?

A. Per unit volume of the chamber?

Q. Yes.

A. Well, very considerably less.

Q. Would it be as small as one-fifth?

A. It might very likely be.

Q. Then, it would be about the same ratio of heating—that is, the amount of heat supplied through the coil, D, as compared with that supplied by the chamber F, would be about the same ratio as in the heating coil in Holmes-Manley, and in the stills in Holmes-Manley?

A. Absolutely not—that is an entirely wrong conclusion to draw from any of my statements.

Q. In the stills in the Holmes-Manley process, you supply about one-fifth the heat that you supply in the coils?

A. That is correct.

Q. And in the Kittle device you would supply about one-fifth the heat in the chamber F, as you would to the coils? [fol. 1172] A. No—you put into your question "per unit volume of the chamber, and the amount in the tube per unit different from the unit volume in the coil, so that ratio doesn't apply to the amount of heat put in—they simply apply to the amount of heat put into the chamber per unit volume of the chamber, and the amount in the tube per unit volume of the tube.

Q. That is per unit quantity of the material contained?

A. No, it is not per unit quantity. The real question is the relative amount of heat put into the two things, and the more important question is what happens to the temperature in the two cases.

Q. You don't know what happens to the temperature unless you know how rapidly it is used up in the chamber F, cracking reaction?

A. You can form a very good estimate of what would happen to the temperature in F, with a lead bath around it, in immediate contact with the chamber, which is 200 degrees hotter than the oil—it certainly is going to be warmed.

Q. You don't know how much hotter the furnace becomes in Holmes-Manley, than the quantity of material in the stills, do you?

A. It is different at different points, as I say. We also have in the stills, exposed areas at both the top and bottom, which we don't dare fire, and that is one reason why we have to put in more heat in the part that we can heat. Here you have the chamber completely surrounded by a lead bath. In the Holmes-Manley stills you have a drop of temperatures, of 40 degrees, or so, and in this you would unquestionably have a marked rise in temperature.

Q. How do you know that?

A. Simply from the fact that you have got a small body that is entirely surrounded by molten lead, at 200 degrees higher temperature.

Q. Yes, and if that chamber F were very large, you would have a very small relative area in which to transfer heat from that lead into the body of gas?

A. I don't need to speculate on how large that chamber is. It is only about four inches in diameter and about six inches high.

Q. And how does the volume of that chamber compare with the volume of the coils, D?

A. The volume, I should judge, from looking at it, was quite a little larger.

[fol. 1173] Q. How many times larger?

A. I would have to figure it. I will have to scale this off.

Q. You can take the figures you have given me, I think will be sufficient.

A. I have just given an approximation. If I am going to calculate anything I had better get it accurate.

The Master: What are you going to do—calculate the approximate measurements from the proportions of these drawings?

The Witness: That is what has been asked for.

The Master: Is there nothing in the patent which gives the relative sizes?

Mr. Wiles: It gives the dimensions.

The Witness: It gives the dimensions of one part, but not the part that he is asking about.

Mr. Wiles: He is going to scale the unknown parts from the known parts.

The Witness: That involves an assumption, of course. The inside diameter is about four and a half inches, and the height is about six inches—judging merely from the

proportionality to the coils used, whose length is given. About ninety-six cubic inches is the volume of the vessel, on that assumption.

Q. I only care for the ratio in volumes between the coil and the vessel.

A. Now, there we will have to make another assumption as to whether the diameter of the tube is the inside, or the outside diameter.

Q. Assume that it is the inside diameter.

A. The volume of the coil is about six and eight-tenths cubic inches, if my figures are correct, as compared with the ninety-six cubic inches volume in the separating chamber.

Q. What are those figures; how do they compare?

A. 96 cubic inches in the chamber and about 6.8 in the coil.

Q. Over ten times as much in the chamber as it is in the coil?

A. Yes.

Q. How does that compare, the relative volumes in the chamber and coil of the Cross process?

A. Well, I don't know. I may have some data that I can figure it from.

Q. I do not think we need take time now. Could you supply me with that information in the morning?

[fol. 1174] A. Yes.

Q. And also for the tube and tank?

A. Now what are you going to call the coil in that—not all the sources from which they pick up heat, but just the fired coil? Some heat is picked up in the dephlegmator.

Q. Well, you might include both of them.

A. Figure it both ways?

Q. Yes.

A. I don't know whether I even have the data to get it if we include the heating surface in the dephlegmator. I think I probably do have the data for the other. At least I can give you a rough figure.

Q. Turning to that Schuchow patent just for a moment, the draw-off pipe D leaves the separating chamber A in substantially the same position that it does in the Clark patent, does it not, from the side of the chamber?

A. Yes.

Q. There would not be any more opportunity for withdrawing carbon through that pipe than there would be in Clark, would there?

A. I should say there would be much less, because I would not expect to have any carbon in A.

Q. Even in you decompose the hydrocarobns?

A. Even if you decompose the hydrocarbons?

Q. Yes.

A. Carbon would be formed back in the tube and stick there.

Q. And none of it would go up into the chamber A?

A. Well, it is conceivable that some of it would be carried along.

Q. That would be true of Clark, too, wouldn't it?

A. Well, Clark is doing his cracking in his bulk supply. That is the difference.

Q. And there would not be any cracking in Chamber A?

A. Certainly not to amount to anything.

Q. Where does the Holmes-Manley do the bulk of its cracking; any of it in the coils?

A. No, it is practically all done in the stills.

Q. Have you determined that of your own knowledge?

A. Yes.

Q. Was that in the Tulsa plant or the Whiting plant?

A. Whiting plant.

[fol. 1175] Q. You do not know whether that holds true for the Tulsa plant or not, do you?

A. I know that the Tulsa plant does still less cracking in their pre-heater than we do.

Q. In Kittle where would the greatest amount of cracking take place? There would be some done in what you call the separating chamber?

A. Yes, in Kittle some of the cracking would be done in the separating chamber.

Q. Have you any idea how much?

A. Well, I should imagine the larger portion of the cracking. It is hard to be sure, because so little is disclosed about it, but that would be my guess, that most of the cracking would be done in the separating chamber if you followed these particular proportions.

Q. Yes. And to maintain a liquid in that chamber F the pipe f^2 ought to project up into that chamber to some extent, ought it not?

A. Well, if the pipe f' projected up into the chamber and if the temperature wsa not so high as to coke everything, then you would have a body of liquid in the chamber. It might very likely turn to coke, of course, at those temperatures, especially at the low pressures indicated.

Q. In the Burton patent No. 1,160,689 the safety valve patent, what is the essential feature of that invention?

A. As I pointed out, the essential feature of that invention is the slight cooling of the large volume of vapors discharged from a pressure still, to get them below their autoignition temperature and yet not condense liquid and spray it all over the place.

Q. In doing that you simply lead it through a small pipe up into the atmosphere?

A. Yes.

Q. And the model of the Holmes-Manley plant that I saw ran it right out under the roof, at the side of the building?

A. Do you mean it discharged under the roof?

Q. It seemed to to me, or about that level.

A. Well, that is probably inaccurate. It either projects through or outside of the roof.

Q. In the petitioner's exhibit No. 80, the Atwood patent No. 28,448, you also have a pressure release valve, do you not? It is indicated as I, but very difficult to see in this reproduction.

[fol. 1176] A. There is one some place. I cannot just spot it at the moment.

The Master: Have you found it, Mr. Rauber?

Mr. Rauber: If I had a microscope I might read it.

Q. Pipe J is provided at the top, and this safety valve opens into that, does it not?

A. Yes, the safety valve discharges into J.

Q. From the top of the still into the horizontal pipe J?

A. Yes.

Q. And that is connected to a vertical pipe J3?

A. It is rather hard to see. Yes.

Q. And that pipe J3 extends up through the roof of the building?

A. I do not see that there is any building.

Q. I think it says so on page 1165, in the left hand column, about one-third from the bottom.

A. Yes, it says it sticks up through the roof of the building.

Q. That construction so far is just the same as your safety valve construction, isn't it?

A. No, not at all.

Q. You have just a pipe leading from the side and up through the roof of the building?

A. Yes, but we do not take it down through a partial condenser and try to save a lot of liquid which would be sprayed around.

Q. The difference is that the Atwood patent shows a little draw-off spigot J2, I think it is, at the lefthand side of Fig. 2?

A. The difference is in saying that "the condensable products escaping through the safety valve are saved in the main J and flow out through the small pipe J2 into any suitable receiver." In other words, he is talking about recovering——

The Master: Oh, no. "The fixed gases and vapors not condensed escaping through the discharge pipe J3, shown broken off, which is carried up through the roof of the building or in any other suitable manner, away from the vicinity of the still."?

Mr. Wiles: Yes.

The Witness: Yes, but he says, "the condensable products escaping through the safety valve"—oh excuse me. "The condensable products escaping through the safety valve and saved in the main J flow off through the small pipe J2."

[fol. 1177] The Master: That is all right for that, but "The fixed gases and vapors not condensed escape through the discharge pipe J3."

The Witness: Yes, that is correct.

The Master: "Which is carried up through the roof of the building or in any suitable manner away from the vicinity of the still."

The Witness: That is correct.

Mr. Wiles: May I suggest——

The Master: No, you may not. We have argued about this now.

Mr. Wiles: I thought from your Honor's point here——

The Master: Mr. Wilson is under cross-examination and able to take care of himself.

Mr. Wiles: All right.

Q. Now, in connection with this Humphreys carbon-collecting plates, I will asy you to refer to page 1170.

A. Yes.

Q. The two cups or buckets marked 42 are for the purpose of collecting carbon, aren't they?

A. Yes.

Q. And the circulation of liquid would be up the side of the still and over the top?

A. Yes.

Q. And any carbor that settled out would settle on the buckets 42?

A. I do not see it here. If it settled into the buckets 42, yes.

Q. That is what they are there for, is it not?

A. They are there to catch what they can, and with that method of circulation they would catch a fair proportion, but by no means all.

Q. That is the same way that the Humphreys collecting plates operate, is it not?

A. Well, except for the difference that Humphreys will work in a pressure still and these won't.

Q. Why won't they?

A. Because you have the big gap between them and no chance of using steam to create artificial circulation, and so the coke simply passes down between the pans. Even a small crack, as I pointed out, is enough to render Humphreys' false right below even a small crack.

[fol. 1178] Q. These buckets 42 have retaining lips to retain the carbon?

A. Yes. I am talking about when you have that circulation, you have carbon and asphaltic matter coming down in the circulating stream and depositing where it changes its direction, which is right at the bottom of the still at the hottest place.

Now, if you had steam there to keep that in violent agitation, you might be able to blow it away, and make it eventually go up into the quite spot, 42. But with no steam that does not take place.

Q. In the Humphreys plates the oil comes up around the sides of the still?

A. You do not have very much interchange.

Q. You do not have very much oil going around?

A. You do not have very much oil going around there; you have some.

Q. Then it comes down through the center between the plates?

A. Well, you do not have much circulation between above the plates and below. You are transferring heat right through the plates.

Q. There is no circulation from below the plates?

A. I do not say no circulation, you would have very little.

Q. Very slight?

A. Yes.

Q. Are those Burton stills ever operated without Humphreys' plates?

A. We have never operated any that way since I have been there. I do not know of any ever being so operated unless possibly for a test. Of course, they were probably so operated for the first few months before they found the great advantage of the plates.

Q. You think they could use steam in such stills to increase the circulation and it would work better?

A. Well, it would ruin the cracking. It might stop the coke deposit, but you would not have any coke to deposit.

Q. Did you ever try out either of these plates in a pressure still?

A. No.

Q. You don't know how well they would work?

A. Yes, I think I do.

Q. Not from actual experience?

[fol. 1179] A. Not from actual experience, no.

Q. You think the circulation would be too rapid?

A. You are talking about using that with steam?

Q. Without.

A. Using without steam the circulation would not be rapid enough to keep the carbon blown off and you would have a large amount of carbon settling down on that unprotected space.

Q. But it would be more rapid than you would have in your Humphreys plates?

A. Yes, but in Humphreys you would not have the large amount of carbon coming down.

You see, in a form like this (Henderson) your oil is circulating around like that (indicating), and then bringing the

coke right down to the bottom of the still and then changing its direction and tending to leave the coke in that space right there (indicating), unless you have some very violent agitation to blow it away.

Q. You think the coke would come up over that retaining lip of that bucket.

A. It would never get into that retaining lip of the bucket.

Q. It would settle out of the oil?

A. You have got a current of oil going around slowly, assuming no steam, up the sides and down the center and that oil is cracking all the time.

Q. Then slowly across the tops of the plates?

A. Yes. That oil is cracking all the time, hence it always has coke in there ready to deposit, and when it comes down the center it is going to deposit quite a little of that coke right on the bottom. Where it changes direction it will tend to leave a little behind, go up and crack some more and leave some more coke behind next time.

Q. It is all a matter of circulation?

A. No, it is not all a matter of circulation. I do not get just what you mean.

Q. The slower the circulation the slower you will deposit the coke on the bottom?

A. There are two things, the first is whether the oil gets in there and gets a chance to deposit coke. The other thing is whether you have a very high velocity jet which would carry that away rather than allow it to accumulate.

Q. Humphreys in his patent mentions a catalytic action of those plates.

[fol. 1180] A. Not in the patent cited.

Q. Not in the Henderson patent but in the Humphreys patent.

A. No, in the patent we cite I think there is nothing said about catalytic action. I may be mistaken.

Q. I see I am mistaken about that assertion. I had some impression there was such a mention in the patent. There is no catalytic action by those plates, is there?

A. I do not think so. I cannot say I have proven it by experiment. There are some people that think that surface cracking effects a slight catalytic action. I do not agree with it. I think the weight of the evidence is in the other direction.

Q. If you were to take the distillate from one of the Burton bulk pressure stills and run it through a bubble tower at atmospheric pressure, would it remove a large quantity of the gum-forming constituents?

A. Well, it would remove some of them but not a very large proportion. It does distinctly improve the distillate in that respect but nothing like as much as under pressure.

Q. There are less gum-forming constituents in the lower ends of gasoline, are there not?

A. Yes.

Q. In cracked gasoline?

A. Distinctly.

Q. About what point do they begin to become appreciable?

A. Well, we probably would never think about gum in a cracked product of the Burton type if we could make 325 end point gasoline or something of that kind. It is mostly between 325 and the end point that we have——

Q. You mean between 325 and 437?

A. Yes. I do not mean to say there might not be a little below 325, but the real problem, is in the heavier part.

Q. Whatever there is below that would not be troublesome, is that correct?

A. That is my impression—I cannot say I have run it in engines to be absolutely sure. I know the gum test on such products is quite low, and I assume that would be reflected by engine performance.

Q. Do you know whether the gum-forming constituents are higher boiling than the upper ends of the gasoline?

A. No, the gum-forming constituents are part of the gasoline, that is, until they have formed the gum. I mean, they distill out right with the gasoline.

[fol. 1181] Q. About what percentage of the gasoline do they form?

A. In what?

Q. In percentage by volume, of the total amount of gasoline?

A. The gasoline made by a Burton still, running on ordinary charging stock?

Q. Yes, or by any of the defendants' processes?

A. Well, I cannot answer as broadly as that. I have never paid a great deal of attention to the precise amount of gum made by the tube and tank process.

Q. Do you know how much there is in the gasoline from the Burton pressure still?

A. You are talking now, are you, about the product obtained if we take the pressure distillate and re-distill it at atmospheric pressure to get a gasoline out?

Q. Yes.

A. Oh, it is a small fraction of a per cent.

Q. You have small fractions of a per cent of higher boiling constituents in gasoline ordinarily, do you not?

A. Well, what do you mean by "higher boiling constituents?"

Q. The hydrocarbons that might boil in the kerosene or gas oil range.

A. If you have a plant with any kind of proper fractionating equipment I don't think you have anything in the gas oil range. Of course, part of the gasoline itself is in the kerosene range.

Q. In an ordinary fired still, in a steam still, you could never get a cut boiling up to 437 without carrying over some higher boiling hydrocarbons, is that not true?

A. I do not want to give a misleading answer. We know this fact: that there are in a gasoline with about 437 end point from the ordinary operation, materials that boil higher than 437.

Q. Yes.

A. Which are a perfectly satisfactory constituent of gasoline. When we talk about the end point of 437 that means the end point determined in a certain way, and part of our heavier constituents have been distilled out with the early part of the gasoline distillate.

Q. That would be a very small amount, would it not?

A. Again, it depends upon what boiling point materials you are talking about.

[fol. 1182] Q. 437 end point gasoline from a steam still or a fire still.

A. Yes, but how much would there be boiling in what range?

Q. In accordance with the Navy specifications.

A. No, but in asking your question you want to know how much material there is that boilss any point above 437?

Q. Yes.

A. If taken out and segregated?

Q. Yes.

A. Oh, there might be several per cent.

Q. You could distill off all of the gasoline and leave most of the gum-forming constituents as a residue, could you not?

A. If you are content with about 325 and point product, yes.

Q. Is not that the way you test for gum in gasoline; slowly distilling off in the air?

A. Slowly distilling off in the presence of air, yes.

Q. Yes.

A. (continued). But during that process, it is not a mere proposition of leaving the gum-forming constituents behind, because they are heavier. During the slow distillation in air you form gum. In other words, you turn your gum-forming constituents into gum, and those themselves are very much less volatile. You can distill in the absence of air one hundred times and still have your gum-forming constituents left in.

Q. At the same rate of distillation and temperature that you would in your open dish?

A. Yes.

Q. Could you distill in any other gas in the same way you could distill in air and still form your gum?

A. Well, air or oxygen or anything containing oxygen would form the gum and make the difference.

Q. I mean, gum-forming materials though are heavy, high-boiling hydrocarbons?

A. Not a bt of it, no sir. They boil right in the gasoline range before they form the gum.

Q. But they do not boil in the lower end of it?

A. Not in the lower end of it, but they cannot be seperated from the gasoline by mere fractionation.

Q. Not even in a bubble tower?

A. No.

Q. Not completely?

A. Well, not in any large proportion, though if you leave [fol. 1183] a five per cent residue, that five per cent residue will contain a larger proportion of gum than any of the lighter cuts, instead of decreasing as you go. But you can frationate until you are blue in the face and still you will find gum-forming constituents will come over, because they are just part of the gasoline cut. They are hydrocarbons

that are less stable and tend to take up air, but they are not heavy.

Q. Would the distillation of a cracked material in the bubble tower of the Barbet patent remove any of the gum?

A. That involves a fresh assumption as to whether you have gum in it or not.

Q. If you had a cracked material?

A. I do not know about the gasoline made by the light cracking to make essentially kerosene. I do not know what its gum forming tendencies are. I assume there might be some. There would probably be if it were made from petroleum and there would probably be from shale oil, but I just want to point out, it does involve that assumption, which I do not know.

Q. You think the light cracking of shale oil products would not give you gum?

A. I do not know. I think it probably would but I cannot say definitely of my own knowledge.

Q. If it gave you gasoline it would probably give you some gum, wouldn't it?

A. Yes, I think if you carry it far enough to get gasoline—in fact, even the gasoline that is right in the shale oil is rather notorious for its tendency to form gum, so that I would expect if you were cracking to make gasoline that you would get gum.

Q. And if you distilled such a material, or a heavy petroleum, at a pressure of about five or six atmospheres, would you form some gasoline?

A. You undoubtedly would form a little gasoline.

Q. On page 5186 of the Typewritten Transcript of the Record, being the testimony you gave on October 12, I find the following statement, containing the sentence: "in the fact that these others who were working on the same problem, had to work for six or seven years before they were able to develop, with all that was shown in the prior art, any other process which was capable of anywhere near competing on a commercial scale, with the Burton."

[fol. 1184] Will you tell me who those others were that you had in mind, in making that statement?

A. Well, take, for example, The Texas Company, who were working on the development of the Holmes-Manley type of process; the Standard of California who were work-

ing to perfect cracking processes during that period, and the Gulf Refining Company, who were working very intensively during that period, and financing some work at Mellon Institute. I assume, from the general circumstances, that no large oil company, with a research staff, could possibly overlook the situation, although I am not familiar with just what went on in their laboratories during that period, but cracking was something that everyone recognized as being quite profitable if you could get a good process.

Q. And this period of six or seevn years would be six or seven years from 1913, the date that the Burton patent came out—was that the idea?

A. I am not quibbling about a year or two.

Q. About that time?

A. About that time—I was thinking of it starting in 1912.

Q. Was the Gasoline Products Company working on that?

A. Yes, they were working over part of that period.

Q. And the Standard of New Jersey?

A. Over a part of that time.

Q. And do you know whether the Gulf Refining Company had a competing process at that time?

A. What do you mean by competing process?

Q. A process which would come anywhere near competing, on a commercial scale, with the Burton process?

A. It has been my understanding that they did not. I have been told that, by some of those who worked on it. I can't say anything on that from first-hand knowledge, or from seeing the cost sheets.

Q. And do you know whether the Standard of California had developed a process that could come anywhere near competing, on a commercial scale, with the Burton process?

A. I don't think they did before 1919.

Q. Do they have one now, that competes?

A. They have several different types of cracking installations. Some of them are really modified Burton apparatus, particularly to take care of this corrosion difficulty which I mentioned.

[fol. 1185] The Master: You are confining your answer to the period anterior to Burton's patent, are you?

The Witness: No, this is for six or seven years after Burton did his development work. He developed the proc-

ess in 1912, and it was six or seeyn years before anyone else got a process which could come anywhere near competing with it. I know that was the statement that was made to me at the Bureau of Mines, when I inquired into the thing, sometime in 1919.

Q. You don't know of your own knowledge?

A. Well, it depends on what you mean by my own knowledege. I have discussed this thing, at that time and since, with many people, in the industry, and I am familiar with the historical development of the various processes.

Q. On page 5723 of the Typewritten Transcript, you mentioned, among the features disclosed by Adams, the application of heat to the intermediate portion of an ordinary cracking still, below the liquid level.

A. Yes.

Q. Will you please point out in what Adams patent you find this? Do you find it in patent No. 1320354—"Exhibit No. 185?".

A. What is the exhibit number on Adams patent No. 1327263?

Q. That is "Exhibit No. 188."

A. I find it, for example, in Adams patent No. 1327263, "Plaintiff's Exhibit No. 188."

Q. What part of that apparatus is this vertical heating?

A. The heat is applied to the chamber 12 at a point below the liquid level and above the bottom, where the tar and scrapings collect.

Q. That is the part that you before pointed out as being the indirect heating element?

A. Yes, as part of the indirect heating element. Of course, in that process he also discloses preliminary indirect heating elements as well. In other words, he does not put all of his heat in at this point.

Q. Have you any idea how much he puts in at this point and how much at the other? The main part of the heat is applied in the furnace around the tube 12, is it not?

A. I am looking for the place where he speaks of how much is put in the pre-heating coil 5. I have not yet located that.

Q. He says it may be brought to any desired temperature.

[fol. 1186] A. Well, that indicates that he might put in a very substantial or certainly a very large portion of the heat

in the preheating coil 5 before it would enter this vertical section which is fired below the liquid level and above the tar receiving chamber.

Q. Mr. Wilson, did I understand you to say that you did not know what the term "intermediate oils" mean as used up to 1910?

A. I do not think that exactly expresses my position. "Intermediate oils" may mean a great many different things, depending upon the context. If they are considering a series of crudes, why, it means an intermediate crude. If they are considering a series of distillates, why, it means an intermediate distillate. I said just the mere term "intermediate" does not have any particular meaning apart from a context.

Q. So far as you know, it has no definite meaning, or did not have up to 1910?

A. No, so far as I know, it has no definite meaning apart from a context.

By the way, do you want those figures on volumes?

Q. Yes, will you give me them.

A. In the Holmes-Manley and the Cross processes, of course, the ratio between the volume of the drum and the volume of material in the coils which are heated in one way or another, will vary quite a little depending on design, but as a rule in the Holmes-Manley process there is around five or six times as much volume in the stills as in the heating coil.

The Master: At a given instant?

The Witness: In a given instance?

The Master: Not a given instance, but at a given instant.

The Witness: No, I am just talking about the volume apart from what is in it.

The Master: Is the volume or the ratio constant during the entire operation?

The Witness: Well, if you talk about the amount of oil that is in there, that is a different thing. I am just talking about the volume of the apparatus.

The Master: Oh, the capacity volume.

The Witness: Yes, the vapacity volume.

The Master: Or volume capacity.

The Witness: Yes, the volume capacity of the apparatus. And in the case of the Cross process the volume of the drum

is from three to four times the volume of the heating coil. There undoubtedly are variations outside of that limit, but that is a fairly typical ratio.

Q. Now, in running a crude, straight-run still, you make your cuts by gravity, do you?

A. In a coking still?

Q. Yes.

A. Yes.

Q. At what gravity do you cut the gasoline from the kerosene?

A. I cannot tell you. I do not carry those in my mind.

Q. You have no idea at all?

A. No, because as a matter of fact, our primary cuts are not gasoline and kerosene, as I explained. Our primary cuts are stove distillate and first distillate, socalled, which overlap in boiling range. That is, our primary cuts on the coking still.

Q. You do not make the gasoline distillate?

A. Not directly from the coking stills.

Q. You make the kerosene distillate?

A. Not directly from the coking stills.

Q. Do you make it from any still?

A. Yes.

Q. What still?

A. Stills that are running for steam refined.

Q. That is a steam still?

[fol. 1187] A. Yes, they are steam and fire stills; most of the heat is supplied by the fire.

Q. Do you know at what gravity the initial point of the gas oil distillate is? That is, do you know the gravity at which they cut from kerosene to gas oil?

A. No, I do not have that figure in mind. I can estimate about where it would probably come, but I do not know.

Q. Give us your best estimate.

A. I should imagine today, making the quality of kerosene that we do today, the gravity of the stream would be around 38.

Q. About at what gravity do you cut from gas oil to paraffin distillate?

A. I do not know. That would be harder to estimate because they put so much gas oil into the paraffin distillate in order to keep balanced viscosity—so much material

which might be considered as gas oil, but which actually goes into paraffin distillate.

Q. Would it be as low as 31 or 32 and still be gas oil?

A. Well, you could still put it into gas oil, but also there is a great deal of such material in paraffin distillate. In fact, the first half of paraffin distillate as made in our refinery today, boils in the gas oil range.

Q. What is its gravity?

A. The distillate as a whole?

Q. No, that part that you put into the gas oil?

A. You mean the part that we take out of the paraffin distillate later and put into the secondary gas oil cut?

Q. Yes.

A. That is about the same gravity as gas oil, maybe a little lower. 32, around in there.

Q. Then the gravities range from 32 up to 38?

A. The gravities of what?

Q. Of the gas oil fraction?

A. No, that would not follow, because this 32 gravity material, which we take from paraffin distillate and put into gas oil itself, covers the range from probably 28 up to 38. That is if we divided that up into fractions it would cover a much wider range of gravity; it covers practically the same range as the gas oil does.

Q. That is, an average gravity of about 32 then?

A. An average gravity of around 32.

Q. What is the average gravity of gas oil?

[fol. 1188] A. The primary gas oil cut?

Q. The gas oil you use to crack.

A. Well, I should say that was around about—do you mean to include in that the naphtha bottoms from the heavy end of the kerosene?

Q. Whatever you do use.

A. Well, of course, our mix includes not only those but it also includes the bottoms from pressure distillate and the overhead from pressure tar distillate. That is the mix that we charge to our cracking stills.

Q. That is re-run stuff?

A. That is straight-run plus re-run, yes.

Q. What would it be if you used just the straight-run gas oil?

A. Including the bottoms from straight run kerosene and the overhead from paraffin distillate?

Q. Is that what you use?

A. We do have those constituents, yes.

Q. What would that gravity be?

A. I should say it would be 33 or 34 in the ordinary ratios that we use.

Q. Could it run lighter than that and still be usable?

A. Oh, that itself contains much lighter material.

Q. But they do have gravity somewhat lighter than 36?

A. Yes.

Q. Or 38?

A. An average gravity of 38? Well, an average gravity of 38 would be too light to handle in the Burton stills. You might use it in some *it in some* other type of stills but not in the regular Burton stills, because there would be so much kerosene in there as to raise the pressure, I am afraid. Of course, you could modify your schedules.

Q. 36 or 37 would be as light a stock as you could use, then?

A. We would not use that. We would get rather slow cracking.

Q. It would be a gas oil distillate, would it not, if the distillate had a gravity of 36?

A. Well, it would not be a gas oil distillate such as Burton discloses, if that is what you mean to imply.

Q. Not boiling upwards of 500 degrees?

A. I should say a 36 gravity mid-continent gas oil, assum-
[fol. 1189] ing a normal cut, covering a normal range would have quite a bit boiling below 500 degrees.

Q. Can you tell us what that 32 gravity would be in specific gravity?

A. I can look it up. (Referring to documents.) 32 gravity would correspond to .8654 specific gravity.

Q. What would the 36 be?

A. 36 gravity would be .8448.

Q. Now, in any of the processes of the Burton group, those owned by the Standard of Indiana, do you ever release the pressure to substantially atmospheric pressure and condense?

A. We have carried that out experimentally but we do not use it, so far as I know, as a commercial process.

Q. It is not what you would call the Burton process?

A. Well, it is not the Burton process as carried out by us.

The Master: The question is about the Burton process as called for in the patent.

The Witness: It is not the Burton process, in the narrow sense certainly, that is described in the patent. It would include certain valuable features—it might include certain valuable features.

Q. In the Holmes-Manley process, as you recall from the reading of the patents, do they ever release the pressure of the vapors to substantially atmospheric and condense them?

A. I do not recall any patents which disclose the Holmes-Manley as a whole, so I do not know what you refer to.

Q. In the Krey process, they do release the pressure of the vapors and condense them at atmospheric pressure?

A. Yes.

The Master: What process is that?

Mr. Rauber: Riebeck or Krey.

The Master: K-r-e-y?

Mr. Rauber: That is right.

Q. That is true, also, of Young?

A. Yes, sir.

Q. And Schuchow et al.?

A. Yes, except for the condensation which takes place in his separating drum, due to the introduction of cold feed, and due to radiation. The final condensation is what we are considering and is at atmospheric pressure.

The Master: And the same way with Dewar & Redwood?

The Witness: Dewar & Redwood condense under pressure, pumping in gas to maintain the pressure—they pump gas into the stillhead, or some other convenient part of the apparatus, whatever they wish, but the pressure is carried through the condenser.

The Master: I understand. I see that there is a valve in [fol. 1190] there that controls the pressure, but that don't imply pressure in the condenser—it was between the condenser and the still.

The Witness: Dewar & Redwood does involve condensation under substantially still pressure, and with the aid of pumping in gas.

Q. You mentioned separating some of the vapors in the Schuchow, et al., patent, by means of cold feed. You have

reference to the separation under pressure of some of the vapors in a dephlegmator?

A. I refer to the action which takes place in that separating chamber immediately above the coil, in the Schuchow & Gavrilow patent.

Q. You mentioned a dephlegmator—does any separation take place there?

A. In the condensation of vapors?

Q. Yes.

A. Yes.

Q. Do they condense heavier or lighter?

A. The heavier portions of the vapors are condensed.

Q. And that is under pressure?

A. Yes—undisclosed in amount, of course.

Q. Now, referring to the patent of Humphreys, Rogers and Paulus—I haven't that exhibit number. I notice by the drawing that the apparatus shown in connection with this process is substantially the same as the Humphreys patent, No. 1119700—is that correct?

A. What do you mean, is that correct? That the two drawings are correct?

Q. Are the drawings, in your opinion, the same—of the apparatus?

A. I will have to refer to the Humphreys patent drawing. Is that the Humphreys run-back patent?

Q. Yes.

A. That is "Exhibit No. 178."

The Master: Do you refer to Fig. 1?

Mr. Rauber: Yes, your Honor.

A. There are a number of quite apparent differences. They can be operated in substantially the same way, or they can be operated in a different way. Thus, Hum- [fol. 1191] phreys, in his patent, shows a valve at the top of the run-backs, and shows the gas release line near the exit of the condenser, instead of down on the receiving drum. But that is not a substantial difference.

The Master: He does not show the pipe 8.

The Witness: I was just going to mention that. He doesn't show the sampling pipe 8, and he apparently doesn't show the pressure release valve on the liquid line

at the bottom of the drum, 8, which must, however, be somewhat beyond where that line is broken off.

Q. Are those, at all substantial differences?

A. I don't think so.

Q. Could you operate the process of this Humphreys, Rogers and Paulus patent, in the apparatus shown in patent No. 1119700?

A. The question is a little ambiguous. If you mean to read into the drawing of Humphreys, the fact that the still is a low pressure still, such as he describes, then you could not carry out this process. But if that still is capable of sustaining the necessary pressure to meet the other process, and if you have a sampling line to determine the composition of the vapors, as specified, I cannot, now, see any reason why you could not carry out the Humphreys, Rogers and Paulus operation in that still, since the H. R. and P. still is diagrammatically similar to that shown in patent No. 1119700.

Q. How much of pressure would be required to carry out the Humphreys, Rogers and Paulus process?

A. Well, that is a very involved question, of course. They cover a range of pressures to accomplish a certain particular result on a particular stock.

Q. What is that range of pressures? Could you give me, roughly, the range, without taking too much time?

A. Well, I say, they set forth in this patent the fundamental principles which enable you to determine the right pressure to accomplish a certain useful result, and, as I recall it, and so far as I have been able to read it now, they do not specify exactly what pressures are required. They do give a definite procedure for very quickly and readily finding out when you have the right pressure for a given stock.

Q. What was the result that they wanted to get—the production of gasoline?

A. The production of gasoline under certain conditions—[fol. 1192] the production of gasoline particularly at a rapid rate, and yet obtaining a good quality of distillate.

Q. Could you operate the process of this Humphreys patent, No. 1119700, without using any of the disclosures of the later Humphreys, Rogers and Paulus patent?

A. Well, of course Humphreys, Rogers and Paulus have a general disclosure of cracking, and things of that kind. Do you mean without operating within the limits set forth?

Q. Yes, without using their contribution.

A. Oh, yes—Humphreys is directed to the low pressure, and low temperature type of operation, which Burton originally developed.

Q. And this Humphreys, Rogers and Paulus patent is for higher pressures, is it?

A. For higher pressures and higher temperatures.

Q. Is it an improvement on the Humphreys patent process? That is, the process patent, No. 1119700?

A. Well, that is one of the things on which it is an improvement.

Q. Just what does it do to this Humphreys No. 1,119,700, patent process?

A. As I pointed out, I think in my direct, the real contribution of this patent is in pointing out for the first time the quantitative effect of temperature on the rate of cracking, and the real function of pressure in a process of this type, where you have high temperature and pressure, and substantial runback.

Q. Does this relation hold good in the process of Humphreys patent, No. 1119700?

A. No, the Humphreys operation is not within the scope of those disclosures that are specified here.

Q. Why not?

A. Because Humphreys does not maintain the specified combination of conversion rates, temperatures and composition of the vapors in the still.

Q. So that in using this Humphreys patent, you would not use those rates of conversion temperatures and pressures?

A. Well, what do you mean by using the Humphreys patent? Do you mean in following out the specific, narrow operation to which Humphreys applied his runback principle? Or do you mean, does the Humphreys, Rogers, [fol. 1193] Paulus patent make use of the broad runback principle which Humphreys first disclosed?

Q. No, I mean the first alternative.

A. In the narrow operation, at 4 to 5 atmospheres, or even at 6 or 7 atmospheres pressure, and corresponding

temperatures, you do not meet the conditions of this Humphreys, Rogers and Paulus later patent, at all.

Q. What pressures and temperatures would you have to operate under, to meet the conditions of that later Humphreys, Rogers and Paulus patent?

A. Well, of course that depends on the stock, and on other conditions.

Q. Suppose you are operating on a gas oil stock?

A. I should say pressures in the neighborhood of 200 to 500 pounds.

Q. If you went below those pressures you would not be within the disclosures of Humphreys, Rogers and Paulus?

A. Well, it might be, for a certain stock—you might find that you were a little below that—but for gas oil stocks I should say to meet their conditions the pressures required would be in the general range of 200 to 500 pounds.

The Master: Aside from the regulation of temperature and pressure, or particularly the regulation of temperature, pointed out in the Humphreys, Rogers and Paulus patent, is there any new step in the process, over Humphreys No. 1,119,700?

The Witness: There is no new separate step in the process. It is a disclosure——

The Master: It is precisely the same progression, and all that he has pointed out is degrees of temperature?

The Witness: He particularly discloses cracking rates eight or ten times as great as Humphreys, and yet getting a good quality of distillate—that is the particular contribution that he makes as an improvement over Humphreys and Burton.

Q. In what terms do you measure that cracking rate?

A. He measures it here in the form of naphtha of 250 boiling point, but also gives the conversion into gasoline of 400 endpoint.

Q. And does that give you the rate of cracking, whether you get 250 or 450, or do you have——

A. That gives the formation of naphtha, in per cent. per hour, of certain characteristics.

[fol. 1194] Q. The percentage of certain charging stock changed into naphtha per hour?

A. The percentage of the material that is in the still during that time.

Q. And what is the rate in the Humphreys, Rogers and Paulus patent—minimum?

A. He gives his table, of course, to cover quite a wide range, from 2.2 per cent of 400° endpoint gasoline per hour, up to 35 per cent of 400° endpoint gasoline per hour. Obviously his preferred conditions are above the lower limit, and you would also find it difficult to operate near the upper limit. I should say the conditions that he was mainly aiming at, were in the intermediate portion of that range.

Q. 2.2 is the lowest limit?

A. That is the lowest limit for which he gives the table. Of course, he is supplying information here, as part of his patent.

Q. You might go lower?

A. And be within?—no, I don't think you would be within the disclosure of the Humphreys, Rogers and Paulus patent.

The Master: In other words, he is giving information as to how to operate for the production of gasoline, in a still of the general type of his former Humphreys' disclosure, to the best advantage?

The Witness: Yes, sir—at a much more rapid rate, and so forth.

The Master: It is about such information as a manufacturer would send out in a circular, as to how to operate a certain apparatus to the best advantage?

The Witness: And, of course, it would require changes in the apparatus that Humphreys used.

The Master: Where do the changes in the apparatus come in?

The Witness: The much higher pressures it would have to stand is the principal——

The Master: How do you know? Of course, the Humphreys is practically the Burton, with the runback in it.

The Witness: Yes, that is correct.

The Master: If the engineer in building the Burton plant built it with sufficient sturdiness he could still operate it and obtain the results that Humphreys, Rogers and Paulus——

The Witness: Later disclosed; that is correct.

Q. How high a rate would you get in the process of [fol. 1195] Humphreys' patent No. 1,119,700—that is, the rate of production of your gasoline?

A. You would get in general around a per cent and a half per hour of 400 end point gasoline. That, of course, necessarily varies a little, but that would be about the figure.

Q. To get a higher rate what would you do?

A. Follow the Humphreys, Rogers and Paulus disclosure.

Q. What would you do in following the Humphreys, Rogers and Paulus disclosure; what would the stillman do?

A. Well, it does not start with the stillman. You have got to start with the building of the still.

Q. If you built your still strong enough.

A. If your still were strong enough, the directions of just what to do are given on page 2 of the patent.

The Master: Would the Burton stills as you originally built them under the Burton patent stand the pressures?

The Witness: No, they would not; not with safety. You might get away with it but you would lose your factor of safety. The patent says, beginning line 28:

"During the conversion an atmosphere of which the vapor of the desired product constitutes approximately four per cent or upwards is maintained in the vapor space of the still. The conditions for maintaining this atmosphere can be determined for any given conversion rate by raising the still contents to the conversion temperature corresponding to that rate"—he is here referring to his table of rates for different temperatures—"and then sampling the atmosphere."

That is through this pipe 8 which is shown on the still.

"If the vapors of the desired finished product do not approximate four per cent or upwards, then a higher concentration should be produced by raising the pressure upon the still until the proper atmosphere is obtained."

Q. In the ordinary Humphreys process you have a run-back which amounts to about eighty or ni-ety per cent of the vapors given off by the still?

A. Yes.

Q. And the residual vapors coming off are about sixty per cent gasoline fractions?

A. Yes, in that order of magnitude.

Q. What is the highest percentage of unsaturated hydrocarbons you will find in any American petroleum?

A. I certainly could not answer that. I have not studied five per cent of the different crudes that are available. I [fol. 1196] know the most important common types of crudes, but I do not know them all.

Q. How low a percentage of unsaturatedness do you find in shade oil, Scotch shale oil particularly?

A. Do you mean the——

Q. (Interposing.) Crude shale oil.

A. Straight shale oil, not once run shale oil?

Q. Yes.

A. Well, I cannot answer that, either. I do not know how long they run.

Q. Do you know whether it would be as low as fifty per cent?

A. I imagine if you ran under conditions so that you got secondary cracking or in effect the result which you get by recracking the shale oil in the re-running process, that you might get below fifty per cent.

Q. What re-running process have you reference to?

A. In the re-running of shale oil to make the once run shale oil that we have already referred to. That increases the saturation of the distillate. In making the original shale oil, in passing vapors through a hot ozone or by evloving them very slowly, I understand you can get a similar change which results in a more saturated shale oil than is obtained by the ordinary processes of distillation, where it is distilled out more rapidly.

Q. Do you know whether the practice in distilling Scotch shales has varied any?

A. Has varied?

Q. Yes, has varied any in the last fifty years?

A. Oh, it undoubtedly has, certainly. There has been changes put into effect.

Q. Changes of what nature?

A. I do not know the details of the industry.

Q. Changes in practice and operation?

A. Undoubtedly there have been changes have taken place. I cannot give you the details of them.

Q. That is just a guess, is it?

A. Oh, I have read descriptions of different types of retort and improvements in retorts, and papers on how better results can be secured than in old practice. It is not a guess as to the fact that changes have taken place, no.

Q. Do you know whether Scotch shales have run as low as forty per cent in unsaturatedness—Scotch shale oils? [fol. 1197] A. Crude shale oils?

Q. Yes.

A. No, I do not know whether they do or not. They might.

Q. Do you know whether any of the American petroleums run over fifty per cent in unsaturatedness?

A. Now you are including in unsaturatedness the asphaltic constituents? I pointed out, of course, that those are generally considered to be still different classes of hydrocarbons.

Q. I am using that term in the sense of materials that will be dissolved in strong sulphuric acid.

A. Well, yes, of course if you count the asphalt as part of your unsaturated you can get most anything in American petroleum. What I referred to as being substantially saturated was the distilled portion, not including the asphalt bottoms, in all the forms of crude petroleum with which I am familiar, and in the commoner forms of crude petroleum.

Q. What is the percentage of saturatedness in a gas oil made from Scotch shale?

A. How made?

Q. By the customary method used over in Scotland. You can select any method you want.

A. Well, I can make it a variety of compositions, depending on how many times I re-run it. If I wanted to re-run four or five times I could probably get a gas oil with relatively low unsaturation for shale oil, but even then I should not expect it to be as low as our straight run gas oils.

Q. How low would it be?

A. I don't know. The figures which I gave in my direct were for typical practice, not for what might be done by some special means.

Q. I do not recall those figures.

A. I gave a lot of figures, some of them quoted from the book of plaintiff's expert, as to the general run of unsaturation of the shale oil.

Q. And shale oil gas would run how low?

A. I don't know.

Q. How high would gas oil made from American petroleum run in unsaturation?

A. I have never seen anything that was more than a few per cent. I suppose some special crudes might give more.

Q. Do you know how high the gas oils of California run in unsaturated hydrocarbons?

[fol. 1198] A. I know the gas oil from some California crudes run five or ten per cent, possibly.

Q. What crude was that?

A. I think that was Santa Maria County crude, the particular one that I have in mind; but I am not sure. It was an emulsified crude oil.

Q. How high in unsaturated hydrocarbons would the crude itself run, measuring by the sulphuric acid absorption?

A. Including the asphalt?

Q. Yes.

A. Oh, it would run very high if you include the asphalt.

Q. How high?

A. Fifty per cent.

Q. How?

A. I should not be surprised if it would run fifty per cent. I do not know specifically.

Q. It would not run eighty or ninety, would it?

A. What crude?

Q. The one you are speaking of, the Santa Maria County?

A. The particular one that I mentioned. I cannot be sure. It has an awful lot of asphalt in it. I never determined unsaturation on the whole crude.

Q. How high a sulphuric acid absorption would you get on Kansas crude, do you know?

A. I do not know but I should judge if you include the asphalt, ten or fifteen per cent.

Q. You do not think it would run fifty?

A. What do you mean by "Kansas crude"? Any crude that is produced in Kansas or the typical Kansas crude which is similar to the average mid-Continent?

Q. I had reference to the highest one you know of, the highest limit.

A. I have never made a survey of the asphalt content, which is mainly what this is being concerned with, of the different crude oils. I know that whatever asphalt is pres-

ent, substantially all of it, will be treated out by an excess of sulphuric acid, and if you test a crude oil it tends to give rather high figures, but it is mostly due to asphalt.

The factors which I have studied have been the distillate products mainly, such as Burton uses.

Q. What is the highest percentage of asphalt in any mid-continent oil that you are familiar with?

[fol. 1199] A. Do you include the Smackover oils as mid-continent oils?

Q. No.

A. Well, of those I am familiar with I should say about twenty per cent was as high as the asphalt content ran, and most of them are lower than that.

Q. Can you give me the names of those oils, any of them?

A. No, I cannot. I can give you our straight pipe line crude, which is the thing we are mostly concerned with; which has a grand average of everything present, but undoubtedly there are crudes there higher than the limit that I happen to be familiar with.

Q. Now, in running a Burton still with or without a reflux, what difference in temperatures would you have in the still? You would have a higher temperature, I understand, in using a reflux than you would use without it for a given pressure?

A. You have got to define the question more than that. Are you talking about a constant pressure, and a constant quality of distillate? You have got to hold a lot of things constant before you can answer that question. Just what do you wish me to hold constant in answering that? The volume of the distillate or the quality of the distillate, any particular——

Q. Well, in the normal running commercial operation?

A. In normal running we do not run wtihout the runback.

Q. You say you used a lower temperature—you would use a lower temperature if you do not use the runback?

A. I cannot answer the question unless you specify what you are going to hold constant. You can run with a runback with a whole range of temperatures, or without the runback you can run with a whole range of temperatures.

Q. But with the same pressure you would use a lower temperature without the runback, would you not?

A. Not necessarily.

Q. You could run at the same temperatures?

A. You may. You could run at a higher temperature, you could run at a lower temperature, the same or higher, whichever you wanted.

Q. I mean, to get a commercial operation?

A. That would not have a great effect on your ultimate gasoline yield.

Q. Could you run as low as 650?

A. If you took long enough.

[fol. 1200] Q. Would that be a commercial operation?

A. Your yield might still be very good, yes. There is quite a difference between yield and cracking rate.

Q. Could you operate the process of the Burton patent No. 1,049,667 at a commercial rate without the runback of the Humphreys patent?

A. Yes.

Q. At what pressure?

A. Four to five atmospheres.

Q. What temperature?

A. What kind of stock are you going to give?

Q. What kind of stock could you use for commercial operation?

A. Gas oil; straight run gas oil, particularly the gas oil obtained from the distillation of the light ends from paraffin distillate.

Q. Suppose you used that stock and your pressure four to five atmospheres for commercial operation, what temperature would you use?

A. Well, you could operate quite satisfactorily around 750 degrees Fahrenheit.

Q. That is just the same temperature as you would use with the runback, is it not?

A. Not on the same oil. I should say, if I had the same oil, I would use a little higher temperature. It depends, however, on the quality of distillate you want to make.

Q. That has nothing to do with commercial operation?

A. Oh, that has something to do with commercial operation but there is a wide range of operations which can be commercial and yet varying the composition of distillate. For example, commercial processes today vary from giving a distillate which contains 20 or 25 per cent of gasoline, up to processes which give a distillate which is nothing but gasoline; yet they are all commercial.

Q. You think that temperature limit given by Humphreys of 650 degrees is too low for commercial operation?

A. That is undoubtedly below the lower limit for American petroleums.

Q. Using gas oil?

A. Well, using any material that I know of in American petroleum.

Q. On page 672 of the record Dr. McKee described a process or an experiment, rather, carried on at 350 degrees [fol. 1201] Fahrenheit, in which he put some petroleum in a glass tube and sealed the tube up and heated the tube and contents to 350 degrees, and when the tube was opened and there was some amount of vapor, as shown by the fact that when it was still in the flame the tip of it puffed out and the glass was opened. Would that happen if he had used a petroleum oil, well petroleum? I am asking you now as to your own knowledge, whether you know it would or not?

A. Well, it may have occurred with well petroleum, but not due to cracking.

Q. What would it be due to?

A. Excess of dissolved gases, for example, which might not have had a chance to redissolve; possibly decomposition of a little sulphur compounds, which might be present, something of that kind; it might be due to any one of a number of things.

Q. If it had been a well petroleum free from gases and sulphur compounds, would it be an indication of cracking to get considerable gas generation?

A. A well petroleum free from gas, including dissolved air?

Q. No more than the ordinary amount of dissolved air.

A. Well, the ordinary amount would be enough to saturate the petroleum, then you might heat it up and cool it down and have an excess of gas, because you had not reached an equilibrium between the gas and the liquid below it.

Q. It would not re-dissolve in a tube like that?

A. It takes quite a lit-le time to reach equilibrium between air and oil unless you deliberately shake it up repeatedly and vigorously.

Q. Suppose you allowed it to stand over night, would it make it re-dissolve?

A. I do not know.

Q. You mentioned a casinghead gasoline as defined in 284 Federal. What is the boiling range of the casinghead gasoline?

A. I do not think that was given in the case.

Q. Do you know what the boiling point and range of such a casinghead gasoline would be?

A. I know what it might be. They will vary, of course, quite a bit.

Q. What is the highest it might be?

A. The highest range or the——

[fol. 1202] Q. The end point, the highest end point you could get on a casinghead gasoline?

A. Oh, I do not know.

Q. On a commercial casinghead gasoline what would it normally be?

A. Well, for one not contaminated with—well, that varies considerably even then. I should say a typically good product that was not contaminated with any of the absorbent oil would boil mainly between 75 Fahrenheit and 250 Fahrenheit.

Q. Between 75 and 250?

A. Yes.

Q. That would not be gasoline, according to your ideas?

A. That would not be a satisfactory motor fuel.

Q. And by "motor fuel" you mean gasoline?

A. Well, the definition which I have given of gasoline, which is what is meant when the word is ordinarily used, is a material which is suitable for general use as a motor fuel.

Q. And this casinghead gasoline then would not answer your requirements for a gasoline?

A. No, it would not. It would not meet my definition of a gasoline, because it would not operate many cars satisfactorily in hot weather.

Q. Did you say that there were no aromatics found in United States petroleums?

A. I do not think so.

Q. Well, are there or are there not?

A. Very little in any of the petroleums with which I am familiar. Of course, as I said, as long as we have not finally

identified any but a very few compounds present, it is risky to say that there are not any there. We do not have any absolute test to tell whether they are or not. But as far as isolating them from any American petroleum as reasonably pure aromatic hydrocarbons, I do not believe it has ever been done. There are certain tests which would seem to indicate that there were certain aromatic compounds there. That is, the way they re-act to nitric acid gives an indication that there may be a certain amount of aromatic hydrocarbons present.

Q. Present in what petroleum?

A. Well, mid-continent petroleum, for example.

Q. And you do not know for sure whether they are or not?

A. I know that there are no substantial qualities, and so far as I know the aromatic hydrocarbons have never been separated and identified postively from American petroleum.

[rol. 1203] Q. As individual aromatic hydrocarbons, you mean?

A. Yes.

Q. Well, that is true of any of the paraffins, too, is it not?

A. Oh, no, we have identified at least fifty of the paraffin hydrocarbons.

Q. But there are thousands of others that you have not identified?

A. Well, there are not so many other paraffins we have not identified. There are lots of naphthenes that we have not identified; but we have separated and identified some napthenes.

Q. Have you identified the aromatics as a class in any of the American petroleums that you know of?

A. Well, I say there are certain reactions which oils give which seems to indicate that among the millions of compounds there are some aromatic hydrocarbons, but I do not consider that conclusive evidence. I think there probably are small amounts of aromatic hydrocarbons in some of our petroleums, and of course we know some foreign petroleums do contain considerable quantities.

Q. What foreign petroleums are those?

A. Borneo is the outsanding example, I believe.

Q. How do they know that Borneo petroleums contain aromatics; do they have a test for them?

A. I don't know.

Q. You do not know how they determine that?

A. I do not know how that has been proved.

Q. But you know it has been proved?

A. I know that is a generally accepted fact in the industry, that Borneo oils contain a relatively large proportion. That is the explanation given for their good anti-knock properties, for example.

Q. Do the California oils contain aromatics?

A. They probably contain some.

Q. You do not know how much?

A. No. Relatively small amounts, however, in any of the ordinary California petroleums.

The Master: Aside from scientific curiosity, what object would there be in separating and recovering aromatic hydrocarbons?

The Witness: Well, certain of the aromatic hydrocarbons are rather valuable products. Of course, we have considerable supply from coal tar. I should say the main reason for [fol. 1204] doing it under present conditions would be from scientific curiosity. If they were large in amount, however, so that they could be readily separated, they would be valuable for use in making dyes, perfumes, explosives, and so forth; and certain cracking processes have been carried out on petroleums largely to form enough aromatic hydrocarbons in order to separate them out and use them commercially. You can by cracking at quite high temperatures produce quite appreciable quantities.

Q. These aromatics, do you consider them saturated or unsaturated?

A. They are neither. I mean they are a separate class of hydrocarbons. That is the way we always consider them in our industry.

Q. Are they absorable in sulphuric acid?

A. Some of them are and some of them are not.

Q. Which ones are?

The Master: Which are these aromatics?

The Witness: Benzol, toluol, xylol, naphthalene, anthracene—compounds of that general type.

Q. Which ones are soluble in sulphuric acid?

A. Of what strength?

Q. The strength used for making the test of absorption in sulphuric acid.

A. Well, I cannot give the whole range. I know toluol is more saluble than benzol, and xylol more soluble than toluol. Beyond that I do not know how they compare. Benzol, as I pointed out, is very little soluble, if you carry out the test as specified.

Q. Toluol would be somewhat more soluble?

A. Yes, toluol would be somewhat more soluble.

Q. And xylol still more soluble?

A. I think so, though I am not sure on that point.

Q. How do the melting points vary on those compounds?

A. I think the melting points drop off with increasing molecular weight. That is, I think toluol is much lower than benzol, and I do not know about xylol.

The Master: The world's supply of these products is now dependent upon the coal industry—the by-product coking industry?

The Witness: Yes, that is the source of practically all of those hydrocarbons. Oil gas tar to a limited extent.

Q. In your classification of cracking processes you men- [fol. 1205] tioned one class of flash cracking processes. Do you limit those processes to those in which oil is dropped on to hot brickwork?

A. No, my classification was a flash or film cracking process, characterized by quite high temperatures, very short time for contact, and the presence of only a drop, a few drops or a film or liquid oil in the process.

Q. In other words, a very small quantity of oil?

A. Well, of course, when you are talking quantities it is relative. Thorpe and Young had a bulk pressure distillation process in a small glass tube, because they were carrying out the cracking in a body of liquid distilling off the products as it was formed. We cannot distinguish them on just the quantity of liquid. It is a question of the relative proportions.

Q. You distinguish by very hot surface, very hot heating surface, and the thin film of oil on it?

A. Yes, a thin film or a spray of oil, or something of that kind.

Q. Can you give me some idea as to the lower limit of temperature that you would have on a hot heating surface in a flash cracking process?

A. Well, I stated that flash cracking processes generally operate in the range from a thousand to two thousand. It might still have the essential characteristics of a flash cracking process but not be very effective at lower temperatures than one thousand.

Q. It would run from just above a dull red to a light cherry red?

A. Well, you are talking a language which, as you know, is not a very accurate one.

Q. It is used for designating temperatures, is it not, roughly?

A. Yes, but no two people agree on the designation.

Q. You have no idea what bright cherry red would be?

A. I have no idea what you might call a bright cherry red or someone else might. I have an idea what I would call a bright cherry red.

Q. What temperature would that be?

A. I should say in the range of 1500 to 1800 Fahrenheit; some place in there. Even that I am not quite positive of. I do not carry those in mind, because it is not a satisfactory temperature scale for a scientist.

[fol. 1206] The Master: Is any such flash cracking process used commercially?

The Witness: Oh, yes, in the gas-making industry it is the general method of cracking. You spray the oil on hot brickwork, for example.

The Master: In the ordinary use of fuel oil for heating purposes, where the fuel oil is sprayed onto a hot baffle plate at the back of the furnace, you have a flash cracking operation there?

The Witness: Well, I should not say so. When you are burning the oil you generally do not consider——

The Master: Burning the gas from the oil, are you not?

The Witness: You are burning the vapors generated. You may get a certain amount of cracking, but that is not an essential part of that process. Mostly the baffle plate is there in order to cause rapid evaporation of the liquid as it is fed in.

The Master: The baffle is there to keep from burning out your apparatus, isn't it?

The Witness: That is true, too, in certain types of installation.

Q. Would you consider graphite a good material for constructing a pressure still?

A. Of what size?

Q. Commercial size.

A. No, I would not.

Q. Up to what size could you use it?

A. I don't know.

Q. You do not know whether you could use it on any pressure distillation work, do you?

A. Oh, I know you could use it on a very small scale.

Q. How large?

A. Why, if you had quite a thick-walled tube I should think you could use it for moderate pressures, say a tube of a couple of inches inside diameter.

Q. How thick?

A. One inch walls.

Q. Heated from the outside?

A. Yes.

Q. And what pressure, how high could you go?

A. Well, that would somewhat depend on the temperature to which I was going to heat the graphite, and I really do not know the strength of graphite at the higher tempera- [fol. 1207] tures. I would want to make some tests to decide just how high pressures would be suitable.

Q. What would be your present estimate as to the highest pressure you could use in a tube of that type?

A. Good quality graphite tube?

Q. Yes.

A. What temperature?

Q. A cracking temperature and pressure.

A. Well, I would not guarantee it and I would want to make some tests before I tried it, of course, but I should judge from 15 to 30 pounds would be suitable pressures for such an apparatus.

Q. 15 to 30?

A. Yes.

Q. Of course, if you made it any larger than that you would have to lower your pressure very considerably, would you not?

A. If you use graphite and fire externally.

Q. In building these stills do you rivet the various plates together—the Burton pressure stills?

A. Yes, that is the ordinary method of construction that we use.

Q. Do you ever weld them?

[fol. 1208] A. We sometimes weld them to stop leaks, but our Burton stills, so far as I know, have never been made of all welded construction, though, of course, higher pressure containers have been made that way. You understand, when I say we have not welded them, I mean that while we often do weld the seams to stop leaks after they are riveted, we do not rely on the weld to carry the load—we rely on rivets for that.

Q. On page 5124 of the Typewritten Transcript of the Record, you made this statement:

"The refiners realized that they had to get busy in some way to increase the yield of gasoline. I think that is an historical fact that is pretty well established. A great many were working in the period from 1908 on."

Do you know of any refiners working in the period from 1908—that is, in 1908, and 1912, on cracking processes?

A. I know that our Company was, and I know that George Saybolt was, and, of course, Adams was.

Q. Saybolt, of the Standard Oil Company ow New Jeresey?

A. Yes, Saybolt was. You are talking about the period from 1908 to 1912?

Q. Yes. Adams was not a refiner was he?

A. No, Adams was not a refiner, but Adams was taking it up with other companies, such as the Valvoline Oil Works, who were interested in the thing.

Q. For the production of gasoline?

A. I think so.

Q. And the Standard Oil Company of New Jersey were interested in producing gasoline at that date, by cracking?

A. In that period—of course, part of that period was before the dissolution.

Q. Any others?

A. I don't know when the Gulf Oil Company started, but they were working in 1912—I don't believe I can state definitely for any others, prior to 1912.

Q. How do you know that Mr. Saybolt was working on the production of gasoline at that time?

A. That is a matter that I have discussed with Dr. Burton, as to how he came to start work, and also, with Dr. Humphreys.

Q. It is purely hearsay, is it not?

A. Yes.

Q. Now, in your direct testimony you stated that the fizzing of a pressure distillate was an indication that it had [fol. 1209] been condensed under pressure. Does the gasoline still fizz after it has been treated with acid and alkali?

A. No, it only fizzes just after you take it, or shortly after you take it out of the pressure system. If you let it stand for a few hours, and do almost anything to it——

Q. Or, if you treat it with acid, or alkali, it no longer fizzes?

A. That doesn't have much to do with it, if you let the gas escape. Of course, if you raised the temperature it would not exactly fizz, but it would give off bubles.

Q. And that would happen with almost any gasoline—casinghead, or anything?

A. If you let it stand around a while.

Q. If you heated it up would you get that bubbling effect?

A. Yes, but that wouldn't be the fizzing that I am talking about.

Q. The fizzing that you are speaking about would not take place after the gasoline has been treated with acid, or alkali, a chemical treatment?

A. No, except that I don't want to imply that that would have anything, particularly, to do with removing the fizzing.

Q. But the fizzing goes on after——

A. In our case it goes on in the separating column, and after that the pressure distillate will not fizz.

Q. Do you think you can treat it with acid and alkali, and still keep it in a fizzing condition?

A. If you keep it under pressure, you can.

Q. But without pressure?

A. No, I don't think you could—unless you rigged up some very special type of treating device to keep it in a fizzing condition. Normally, any onsiderable agitation at atmospheric pressure will change it, so that it no longer fizzes.

Q. In the ordinary laboratory test to clear up a sample, that would destroy the fizzing properties, wouldn't it?

A. By the time you finished the treating I should say fizzing would be destroyed. Of course, it might show that fizzing when you shook it.

Q. But after the ordinary acid treatment it wouldn't fizz, would it?

A. No, not in the sense that I was using the term.

Q. Now, in regard to the Chamberlin patent, as I understood your interpretation of that patent, it simply says that you can use a stock that Burton does not disclose?

A. He discloses that in addition to the Burton stock you [fol. 1210] can put in quite a bit of heavier material than Burton discloses as being possible, and still crack in a direct fired still, under specified conditions, and not getting a hot bottom.

Q. You say you can use things that Burton said it was not possible to use?

A. No, I said that he used things which Burton didn't say was possible. Burton limited himself to disclosing one stock which could be used. He didn't say whether others were possible, or not.

Chamberlain said that certain things were possible, which Burton said nothing about.

Q. What were those stocks?

A. Light or heavy paraffin distillates added to gas oil.

Q. Burton did not disclose those stocks, then?

A. Burton disclosed a gas oil stock, and showed its boiling point range.

Q. You mean the true boiling points, even though they might be decomposed, if you tried to run a laboratory distillation?

A. You can't distill them in an ordinary laboratory distillation. You have to use a vacuum, or steam still, or something, if they are much above the six or seven hundred that Burton was talking about—that is, the heavy ends of them are.

Q. Don't they distill those over in atmospheric stills?

A. With steam, yes, or with other means for carrying them off. They run them in coking stills, and you have the formation of gases and light products which act like steam, and, furthermore, you are there distilling them at atmospheric pressure. Thus it is impossible to determine the

true boiling points at atmospheric pressure, although you are still distilling them.

Q. Up to what temperature can you distill the materials?

A. Without getting substantial cracking?

Q. Yes.

A. In a small laboratory still the limit is about 750 degrees—you will get a little cracking, but not enough to hurt your result.

Q. These materials in Chamberlain run above that limit, do they not?

A. In a considerable part, yes.

Q. They are a dirtier stock than Burton specifies—is that right?

A. Yes.

[fol. 1211] Q. I understood from your direct testimony that you stated that Young advises against the use of a water tube boiler—is that correct?

A. Yes, sir.

Q. Were water tube boilers known in 1865?

A. I think they must have been.

Q. Why?

A. Partly from what Young says. I can only quote what he says.

Q. On page 1257, about the middle of the page, he says, "But I found that an ordinary steam boiler, not tubular"—is that what you had reference to?

A. Yes.

Q. That would also describe fire tube boilers, would it not?

A. I suppose it would, yes.

Q. So it might have been a fire tube boiler that Young had reference to?

A. Yes, that is true.

Q. Now, in connection with the Lewis and Cook bubble tower patent, is the tower construction substantially different in the Lewis and Cook patent that what it is in the Barbet patent?

A. It is undoubtedly different in one important detail. It was built to operate under pressure, whereas Barbet's was not.

Q. That is a matter of strength of construction, is it not?

A. Yes.

Q. Heavier constructions?

A. Yes.

The Master: Is there not something more in this case; is it not a matter of identity of process?

Mr. Rauber: This is really a preliminary sort of question. I will get to the point in a moment.

The Witness: Lewis and Cooke has, of course, a process patent, which indicated——

Q. Without going further into that question, taking an individual plate, is the mechanical function of that plate in the Barbet and in the Lewis and Cook tower the same?

A. So far as the fact that you ahve a device which forces the vapors to bubble through pools of liquid, they are similar. The particular type of cap construction is, of course, quite different and I do not think Barbet's particular cap construction would be very suitable for Lewis & Cook's purpose; but in a broad sense the function of the plates in causing the vapor to bubble through the pools of liquid of the plate are similar.

[fol. 1211½] Q. The condensing——

The Master: They perform a similar step?

The Witness: Yes.

The Master: In the operation, but in a slightly different manner, is that it?

The Witness: Well, it is quite a different operation, that is the biggest thing.

Q. They accomplished this step of taking out the heavier portions of vapors going through, did they not?

A. Yes.

Q. And at the same time evaporating whatever light portions there may be in the liquid on the plate?

A. Yes. They accomplish that function.

Q. That is the normal function of a bubble tower?

A. Well, there is one of the functions. Bubble towers are used for many purposes.

Q. What other purposes?

[fol. 1212] A. For stopping entrainment, for scrubbing gas; scrubbing a soluble gas from another gas.

Q. That is the same, is it not?

A. No, you are not trying to evaporate any light ends out of the liquid, in that case when scrubbing a gas; nor are you trying to do it when you use it to stop entrainment.

Q. It is used wherever you want a series of contacts between the gas going out and liquid going down, is it not?

A. Well, it is sometimes used in cases such as those and sometimes not; and it is sometimes used in other cases.

Q. In operating a Burton still of the type shown in Patent No. 1,049,667, you get over considerable condensate that is not usable as gasoline, is not that right?

A. You can operate under those conditions and that is normally what would happen, I should say; or you might operate very slowly at a temperature such as to get a good quality of distillate.

Q. And if you do get in any heavy ends in the gas oil range you can use them for re-cracking?

A. The heavy ends of the distillate?

Q. Yes.

A. Yes.

Q. Are they approximately the same as your re-run material that it is used for re-cracking stock?

A. Oh, no; they are far different in composition; far different.

Q. How are they different?

A. Well, they do not have anything like the unique gravity-viscosity relationship that the pressure tar has.

Q. Have you measured them as to that?

A. Yes. They are much lighter constituents; they are essentially cracked kerosene.

Q. They are in the kerosene range?

A. Yes.

Q. It is more a matter of temperature or of their boiling point range?

A. No, they are products of cracking that have been formed and distilled out practically as soon as they are formed; whereas, the material in the pressure tar has been kept there and stewed and digested and it has undergone quite profound changes. I should say the tar is considerably more different from the type of hydrocarbons you start with than the heavy pressure distillate is.

[fol. 1213] Q. None of that re-cycle stock ever distills out through your condenser in the pressure stills?

A. What do you mean by that? Our re-cycle stock is what we get off from the atmospheric distillation of pressure tar.

Q. None of that material ever gets out through your condenser in the pressure still?

A. Naturally none of the material which goes off as pressure tar, has gone off as pressure distillate.

Q. I mean material of the same character, hydrocarbons?

A. Well, the unique character of the pressure tar, as I pointed out, tends to disappear at the higher Baume gravities, that is for the lighter oils. Undoubtedly, of course, there are some hydrocarbons in the lightest end of pressure tar, which are similar to those in the heaviest end of pressure distillate, but by and large, the two cuts are very distinct in character.

Q. In connection with the Pielsticker patent, you mentioned the use of a steam ejector?

A. Yes.

Q. Was it your impression that that was used during distillation, during the pressure distillation.

A. Well, I do not know that I had any very definite impression on that. He provides an apparatus and he provides a steam ejector for his tar.

Q. Yes.

A. (continued). I presume if he did not need the steam ejector he would not use it, but he does provide a steam ejector there to aid in drawing off tar.

Q. It is only used after the process is ended, is it not?

A. No, it is a continuous process.

Q. He says it is used only after the process is ended, does he not?

A. I do not think so. What exhibit number is that?

Q. I think it is Exhibit 133 or 134.

Mr. Royce: Yes, it is either 133 or 134.

Q. Take 134 on page 1671, lines 30 to 35.

A. (Referring to exhibit.) No, that is an entirely different thing. That steam is for cleaning out the coil as well as the still at the end of the operation.

Q. Yes. That is what the steam jet is for, is it not?

A. No, that is the steam jet at the inlet to the coil.

Q. Yes.

A. I was talking about the steam ejector at the dis-
[fol. 1214] charge from the still; the steam ejector for example in Figure 9; the steam ejector—well for example in

Figure 4, the steam ejector Q just beyond the valve P, which withdraws the tar from the still. The device you are referring to is not a steam ejector at all, it is merely a steam connection.

Q. Yes.

A. (continued). For forcing steam in.

Q. Would that steam ejector Q have any effect on the operation being outside of the still and outside of the pressure release pipe P?

A. No, it is simply an indication of the character of the product produced.

Q. Very thick, heavy product?

A. Yes.

Q. Now, in regard to that Burton safety valve again, does it make any difference where that is mounted on the still?

A. I do not know just——

Q. Any part it could be mounted, any part along the top portion of the still; I mean as to length?

A. The length along the still?

Q. Yes.

A. I should think so.

Q. What difference would it make?

A. I do not think it would make any particular difference unless you wanted to get it as far away from the firing end of the still as possible.

Q. Suppose you mounted it on the vapor pipe, just at the entrance of the still, would that make any difference?

A. Take it out of the side of the stillhead?

Q. Yes.

A. You could take it out of the side of the stillhead undoubtedly. You would not want to go very far along the vapor pipe because you would have additional friction, which you want to avoid.

Q. You might go a little distance if you made the pipe large enough?

A. Yes.

Q. Can you tell me what the refining losses were in the use of sulphuric acid prior to 1910?

A. Use of sulphuric acid on what?

Q. On any of the products.

A. Well, I can only tell by assuming that where the products are similar the losses are similar to those now in-

[fol. 1215] curred, but I do not know the details of their refining operations at that time, prior to 1910.

Q. Do you know what the refining losses are today on petroleum products from straight-run distillation and the use of sulphuric acid?

A. I know some of them in a general way, yes.

Q. How high do they run?

A. On what kind of products?

Q. Any kind you want to select, the highest.

A. Of course, if you are treating cylinder stock to treat out the asphalt, why, you get very considerable losses; 25 per cent or so, anyway.

Q. On treating a material like Nujol you go way up high, do you not?

A. In treating a material like Nujol, even though you start with a pretty good product, you treat it quite heavily, with acid which is of course very strong, to make a white oil.

Q. How much do you lose in treating it?

A. Why, as I recall it, fifty to sixty percent, depending—and even outside of that range, depending on the kind of oil you start with, the viscosity.

Q. What do you remove in that treatment?

A. I don't know.

Q. Do you remove all of the unsaturated?

A. Undoubtedly and a great more besides.

Q. What more? You have no idea what else you take out?

A. I don't know what it is, I confess.

Q. Coming down to twine oil, what is the value of -his oil in the industry?

A. Well, the main thing for which we used it is, of course, as charging stock. It is of value as a twine oil, which is only a limited outlet for the product. It is not good for lubricants or for most of the ordinary uses because it is quite different in character and not highly stable.

Q. Do you know whether any of the defendants other than the Standard Oil Company of Indiana sell that oil as a twine oil?

A. I do not know whether they do or not, no.

Q. So that its importance is almost solely as a re-cycle stock?

A. I do not know just what you mean by its importance. That is its principal use.

Q. Yes. You do not know of any other use of any conse-
[fol. 1216] quence—that is, used by the defendants other than the Standard Oil Company of Indiana?

A. We use it in making up certain of our specialty products.

Q. I am not speaking of the——

A. (Interposing.) I do not know of any other use by companies other than our own.

The Master: It got its name from being used for twine, did it not?

The Witness: Yes.

The Master: That is where it got its name as twine oil?

The Witness: Well, that is where the common designation of the patent is concerned. I do not mean to say we call it twine oil in the refinery; we refer to this as the twine oil patent.

The Master: Twine oil got its name from being developed for use by manufacturers of binder twine, did it not?

The Witness: Yes.

Mr. Wiles: Commonly called pressure tar distillate, isn't it; in the plant?

The Witness: Yes.

Q. Do they use any other petroleum oils for twine oils?

A. Undoubtedly.

Q. Has this any particular advantage?

A. Yes.

Q. What?

A. It gives heavier gravity for its viscosity than others.

Q. How much difference does that make per gallon or per barrel; what percentage difference?

A. Not a very large percentage.

Q. One per cent?

A. Oh, I think it is more than that. Of course, it depends on what you are comparing it with.

The Master: What difference in weight to a ton of binder twine?

The Witness: I don't know enough about the quantities used to answer that. It would be a good many pounds, though, for a given volume of oil.

The Master: Is a large part of the twine industry adjacent to Whiting?

The Witness: I could not tell you that. I have never gone into the marketing of this product for twine uses at all.

Q. The only matter of importance for the licensees of the [fol. 1217] Standard Oil Company of Indiana, as far as this patent is concerned, then, is that it might prevent them running a re-cycle stock?

A. I don't know what they may want to use it for. It is a new product; so they might find quite different uses.

Q. So far as you know they do not use it for anything else but a re-cycle stock?

A. Yes, but I have never inquired into what they do use it for. I don't know. Whenever we find a new product of this kind we find that people do develop new uses for it.

The Master: Do you know how much or how little is sold to the twine industry?

The Witness: No, I do not. That is a matter that the sales department is concerned with rather than the refinery.

The Master: Or if any at all?

The Witness: Oh, I know that some is sold.

Q. Do you understand that this material was made whenever you made a recycle stock by the re-run patent—it is exactly the same material, isn't it?

A. Well, the same range of materials. It is a type of product, of course, we make whenever we operate the re-run—no, we don't always, because sometimes we may separate it into light and heavy; but of course those two are put together.

Q. You might make a re-run stock, then, without making this product?

A. As a general rule in all normal operations, we do secure the twine oil product when we operate the re-run patent.

Q. And so far as you know that it is only use- in re-running as used by the licensees of the Standard Oil Company of Indiana?

A. I do not know one way or another, just how they do use it.

Q. Transforming viscosities from Engler to Saybolt you get the viscosity expressed for the same temperatures, do you?

A. If you can. I mean the ordinary conversion table gives you, of course, the viscosity of the oil at the same temperature.

[fol. 1218] Q. At what temperatures is the Engler viscosimeter operated?

A. A variety of temperatures.

Q. What is the usual temperature?

A. I don't know. I know twenty degrees centigrade is very often used, but by no means invariably.

Q. Isn't that the use specified for that in all text books?

A. I don't think so.

Q. Do you know of any other temperature specified for it?

A. I do not see what you mean by specified for it. It can be operated at any temperature that you want to operate it at.

Q. In making a test at what temperature do you usually operate it?

A. It depends on what temperature you want the viscosity of the oil, the same as in the case of Saybolt. We operate it at many different temperatures.

Q. And then you state the temperature, do you not?

A. Yes, as a rule.

Q. In finding an Engler viscosity without the temperature stated, is it not generally understood that the temperature is twenty degrees centigrade?

A. Not that I know of.

Q. You would not know what the temperature was of Saybolt?

A. No.

Q. It might be 200?

A. It might be 200. But you could generally tell from the context what it was.

Q. Have you ever seen temperature other than 20 degrees centigrade stated for use with an Engler viscosimeter?

A. I think there is no question but what we use it every day at other temperatures, regularly, in our laboratories.

Q. The Engler?

A. Yes.

Q. Do you use the Engler as well as the Saybolt?

A. Yes. Practically the whole industry uses the Engler viscosimeter, as well as the Saybolt.

Q. And if no temperature is specified you have no idea what is meant?

A. Not unless there is something in the context to tell me what temperature is meant.

Q. Then a certain definition in Engler means nothing to [fol. 1219] you, because you do not know the temperature unless you find it in the context?

A. Yes.

Q. And if you find nothing in the text it means nothing?

A. If you find nothing in the text you could not tell what he really meant.

Q. You would not know what the viscosity was?

A. You would know what the viscosity was at some unknown temperature, that would be the unfortunate part of it.

Q. And you would usually assume it to be the standard temperature, would you not?

A. Well, that involves an assumption that there is a standard temperature. For example, we very frequently—I suppose we run more Saybolts at 100 degrees than any other temperature, but no one would think——

Q. (Interposing.) I am not speaking of what you do, but what is generally understood to be the temperature at which the Engler viscosimeter is used.

A. The industry as a whole uses the Engler viscosimeter at different temperatures.

Q. Is there no recognized standard for the temperature on Engler viscosimeters in text books?

A. Well, I have never looked through the text books to see that, so I cannot be sure. All I do know is that it is used at different temperatures, and if I did not see the temperature I would not know what temperature was meant, unless there was something in the context to indicate.

Q. The text books and others give no standard temperature?

A. I say I don't know; I have never looked through the text books to see if it is most generally used at a certain temperature. I am simply stating the facts as they exist in the industry, as to how it is used.

The Master: No matter what your temperature is, the Engler viscosimeter will give you the viscosity for that temperature; is that it?

The Witness: Yes, that is it exactly. We have the same problems in gravities; we have to specify the gravity at a certain temperature.

The Master: If you want to get a comparison of the viscosity of that with something else, you have got to convert the temperatures?

[fol. 1220] The Witness: The only thing you can do is to convert it to results on some other instrument at the same temperature. You do not know what it might be on some other instrument at some other temperature.

The Master: That is what I said.

The Witness: Yes, that is correct.

Q. Viscosity varies with temperature, does it not?

A. Yes.

Q. How?

A. It tends to decrease with increasing temperature.

Q. Speaking of different families of oils, as I understand it the viscosity increases with the gravity?

A. In a common family. The viscosity increases with increasing specific gravity, of course.

Q. Yes, increasing density.

A. Yes.

Q. But there are families that have much less viscosity for a given density than others; is that right?

A. Yes, that is correct.

Q. For example, benzol has a low viscosity, does it?

A. Benzol has a low viscosity for its density, yes.

Q. A relatively low viscosity for its density?

A. Yes, compared with petroleum products.

Q. And toluol also has a heavy density for its viscosity?

A. I should assume so. I do not recall the exact figures, but I should assume so.

Q. And if you had a petroleum product containing a large percentage of benzol in it, the mixture would have a very low viscosity for its density; is that right?

A. Yes, I pointed out——

Q. What?

A. I pointed out you could make mixtures of that kind which would have abnormally low viscosity for their density, even though they were quite different types of materials.

Q. And if you had materials stated as running between certain viscosities, and certain densities, and you knew

that benzol and other materials were mixed in there in all sorts of proportions, you could not assume that the lightest density material had the lowest viscosity, could you?

A. Oh, you could undoubtedly cook up an artificial family that would not have the ordinary smooth trend that you get in any series of petroleum products.

[fol. 1221] And that would happen by a certain larger or smaller quantity of aromatics, such as benzol and toluol?

A. Well, it would take a very large quantity to make any substantial change, and it would mean that you would have to have a lot of aromatics in one particular boiling range and then drop off to very little in some other boiling range.

Q. For instance, if you mixed a large quantity of aromatics, benzol and toluol, with a heavy viscous petroleum product, you would get a heavy non-viscous product; is that right—a heavy product with a low viscosity?

A. Yes.

Q. And its viscosity might be as low as the lowest paraffin product in a certain range?

A. Yes, I should say that was true.

Q. And its density as great as the density of a much higher paraffin product?

A. Probably.

Q. Those aromatics might be obtained from petroleum products, might they not?

A. I do not believe you could—not from any American petroleum. As I have said, efforts to isolate them have not been successful.

Q. In discussing the products of atmospheric pressure, bulk distillation cracking, you would not get any aromatics, would you?

A. From cracking what?

Q. From cracking American petroleum—in your experience with that type of process, you never found any aromatics, did you?

A. There are certain evidences, as I say, that there is a small amount of aromatics.

Q. Negligible amounts?

A. A few per cent, possibly.

Q. What was the purpose in using sulphuric acid in refining products from straight-run distillation?

A. It was generally considered as being to remove the unsaturateds. In other words, what was accomplished was the removal of unsaturateds, and the improvement of color and odor, and those were generally considered to be cause and effect. I am speaking, now, of the lighter products.

Q. Kerosene?

A. Yes.

Q. When you treat cracked, or uncracked material with [fol. 1222] sulphuric acid, what sort of appearance does the sulphuric acid layer have?

A. Of course that depends on the strength of the acid, the temperature, and so forth.

Q. Suppose you use a strong acid?

A. If you use strong acid, you would get brown, or sometimes up to black colors in the acid.

Q. A tarry sort of material?

A. Yes.

Q. And that is your resinified material?

A. It has been resinified by the action of sulphuric acid, yes.

Q. Suppose you had a distillate such as kerosene, containing 17 per cent of unsaturateds, how much sulphuric acid would you require to remove all of them?

A. I don't know. That would vary a lot, depending upon conditions.

Q. Could you do that with one or two per cent?

A. I don't think so.

Q. Or with five per cent?

A. It would take quite a lot to remove them all.

Q. You could remove the portions that are easiest to remove, with one or two per cent of acid, could you?

A. I don't know whether you could say that it was the easiest to remove, or not.

Q. The most objectionable parts, I suppose, would be removed with one or two per cent?

A. That depends on what purpose you wanted it for.

Q. Kerosene?

A. I have seen kerosene, for example, where the first treating did very little good, and it was only in the last clean-up that you could get burning oil.

Q. That was straight-run kerosene?

A. No, that was cracked kerosene.

Q. What would be the effect of resinifying olefins in a kerosene?

A. Well, I presume that if you had resinifying olefins in a kerosene, that they would deposit gum, on a long period of storage.

Q. Wouldn't they be apt to clog a wick, if they had gum in them?

A. It would probably settle down in the bottom of the can—very likely.

[fol. 1223] Q. Then they would not clog the wick?

A. The constituents, before they formed the gum, I don't believe would clog the wick. I don't really know.

Q. But after the gum was formed, it would?

A. If the gum had formed and had not settled out, which is a rather improbable assumption. It tends to settle out in whatever container you have in it, unless you use some agitation.

Q. If you used it in an automobile it might clog the carburetor?

A. Yes.

Q. But it would not clog a wick?

A. It probably stands in the carburetor and is subjected to the air for quite a period of time, and forms gum right in the carburetor.

Q. Suppose it stood in a lamp for a long period of time, it might clog the wick, wouldn't it?

A. It might, and it might settle down to the bottom of the lamp, however.

Q. Is there anything else that would be apt to clog the wick?

A. Oh, yes. One of the most frequent things that clogs the wick is what they called floc—I won't say that it is so frequent nowadays. We generally get it out. But a little bit of suspended matter tends to gum the wick.

Q. That is a gummy material?

A. No, it is a colorless material—I don't know just what it is.

Q. Is it of a sticky character?

A. It is what they call floc—they had a floc test—that is in straight-run kerosene—it is not the brownish gum that you get in cracked products.

Q. It is not associated with colored or tarry materials?

A. Not the ordinary fire that I am thinking of. That is a very old test. It used to be one of the permanent specifications of kerosene, but not so much emphasized of late, because the refiners can get that out fairly readily.

[fol. 1224] Q. In our discussion of the Burton patent this morning, I am not sure that I understood you to say that Burton disclosed that you could use gas oil or that you should not use anything but gas oil; what is your opinion on that?

A. Why, it is perfectly clear on the face that he disclosed the fact that you could use gas oil and get a satisfactory operation.

Q. He did not teach not to use anything else?

A. Well, I do not know just what you mean by that. What he did teach was what you could use. He taught a product that could be used. He did not attempt to say whether or not you could use other products.

Q. Of course, crude oil contains gas oil?

A. Oh, certainly.

Q. And you think, though, that when he taught you to use gas oil he meant to exclude other things?

A. Why, I don't think he meant anything particularly one way or the other. He mentioned and used this. He had not surveyed the whole world to find out just where the lines had to be drawn. He taught you that you could use gas oil and get a satisfactory operation.

Q. He did not teach you that you could not use other things and get a satisfactory operation?

A. No.

The Master: If you did not use gas oil——

Mr. Rauber: How is that?

The Master: If you did not use gas oil and you made any deviation from the steps of Burton's process, then you would not have Burton. Is that perfectly plain?

Mr. Wiles: Certainly.

[fol. 1225] Mr. Rauber: That is perfectly plain, that if you used anything but gas oil with your gas oil it is not Burton.

Mr. Wiles: No. You mean with your gas oil——

Mr. Rauber: Well, crude contains gas oil.

The Master: Yes. If you used the material they had, gas oil mixed in with heavy bottoms——

Mr. Wiles: Oh, no.

The Master: You would not be realizing Burton's invention at all.

Mr. Wiles: Certainly not.

Mr. Rauber: Sup-ose you had gas oil mixed with something else?

The Master: If you had it mixed with much else you would not be using Burton.

Mr. Wiles: Then you would get your question of chemical equivalents. So far as Burton's disclosure itself is concerned, your Honor's statement is entirely in agreement with our position.

The Master: There are some few things that I think are admitted by both sides about the Burton patent.

Q. Mr. Wilson, I am calling your attention to pages 292 and 293 of the 1890 edition of Thorpe's Outline of Industrial Chemistry. That is a standard book of industrial chemistry, is it not?

A. Well it was; it is not today.

Q. At that date it was; was it not?

A. Yes.

Mr. Wiles: What is the date?

Mr. Rauber: 292 and 293.

The Master: What is the date of the publication of it?

The Witness: 1898.

Mr. Rauber: 1899 I think is correct.

The Witness: Copyrighted 1898, reprinted with corrections 1899.

Q. On page 293, in describing treatment of kerosene, he points out that it is treated with from one to two per cent of concentrated sulphuric acid. If you had a kerosene containing twenty per cent of unsaturated hydrocarbons would that one or two per cent of sulphuric acid remove all of the unsaturates or all of the olefins?

A. No, but of course he is not recommending it for that kind of operation.

[fol. 1226] Q. In recommending it for the removal of tarry matters that would clog the wick, is that not right?

A. Yes, but not from that type of a product. He is describing the ordinary straight-run product, as I judge.

Q. What percentage of clefins in a kerosene of say twenty per cent clefin content would one or two per cent of acid remove?

A. How strong?

Q. Concentrated, 95 per cent?

A. 95 per cent?

Q. 93 per cent.

A. 93 per cent?

Q. Yes.

A. I do not know.

Q. Would it remove as much as five per cent of it?

A. I think it would depend quite a bit on the clefins. I think you might easily remove five per cent if they were of a rather reactive type, including the gummy material that is thrown down at the bottom, which can readily be separated.

Q. If it were a straight kerosene do you think you could remove as much as five per cent?

A. In ordinary straight run kerosene you would not have five per cent of clefins.

The Master: Would it remove five per cent of what were there?

The Witness: It would remove practically all of the clefins which were present in ordinary straight run kerosene.

The Master: I think that is what he is trying to get at.

Mr. Rauber: No.

The Master (continued): Would it remove five per cent of what were there?

The Witness: Oh, it would remove practically all of the small amount of clefins which would normally be present in straight run kerosene.

Q. On page 1424 of the exhibits, Exhibit No. 115, Allen gives a bromine number for burning oils from American petroleum as 34.8. About what percentage of unsaturates or clefin-hydrocarbons would that correspond with?

A. I couldn't tell. As I pointed out, the iodine and bromine numbers are not an accurate measure of unsaturation at all in petroleum hydrocarbons.

Q. You could not tell within one or two per cent?

A. Certainly not.

[fol. 1227] Q. They do indicate the presence of clefins, though, do they not?

A. No. You can get an iodine number or a bromine number, particularly with some of the older methods, with compounds that are pretty well saturated; but, as a rule, the numbers are very small, if the compounds are really substantially saturated.

Mr. Rauber: I have no photostatic copy of these two pages and I shall get them and offer them tomorrow morning, if that is agreeable; two pages of Thorpe's "Outlines of Industrial Chemistry."

Q. Referring to Ellis product patent No. 1,318,060——
A. Exhibit number, if you have it handy?

Mr. Royce: Exhibit No. 170.

Q. Is the product described in that patent the result of a bulk pressure distillation?
A. No, it is not.
Q. Under what class of processes it is, or was it made?
A. It is a low temperature tube cracking operation.
Q. How does the clefin content of those products described in Ellis compare with that of Burton—of the products made in the Burton process?
A. It is a little higher in clefin content. Of course, it is lower than the ordinary high temperature type of tube cracking; it is lower in unsaturation than the product of the ordinary higher temperature tube cracking processes, but it is somewhat higher than that of Burton, although not greatly higher.
Q. How does it compare as to quality of clefins with the product made by the Cross process?
A. I should think it would be of the same order of magnitude.
Q. Do you know for certain?
A. No, I have not made a careful determination of the products of the Cross process.
Q. How does the product of this Ellis patent compare with the product made by the tube and tank process?
A. I should—well, in fact, I know that it is not far different.
Q. Higher or lower?
A. It is about the same. I mean, it is in the general range of fifty to seventy-five or eighty iodine number, as a rule.

Q. And from that you determine the quantity of unsaturates or clefins——

[fol. 1228] A. No, from that I determine——

Q. Or estimate it?

A. I do not know how much clefins that corresponds to from that test, but so far as can be told by that test that means the two products are substantially similar.

Q. How does the Ellis product compare with that made by the Holmes-Manley in respect to clefin content?

A. It is a little higher in clefin content, not much. That is, when I am speaking of the Ellis product I am speaking of what he apparently considered his typical product with an iodine number around 75.

Q. Yes.

A. (continued). The iodine numbers of our ordinary cracked distillates are a little below that, as a rule.

Q. How does the clefin content of Ellis compare with that produced in the Young process, as shown in Exhibits 94 to 96?

Mr. Wiles: The Thorpe and Young article?

Mr. Rauber: Yes.

Q. That is the Young process, is it not, the product produced there?

A. No. Well, I should not so consider it. I mean I classified them as different processes, you will recall. They were processes which were designed to find out what happened in Young's still, but they condensed under pressure and they did many other things differently from the way they were done in the Young process.

Q. And how do the products described in those articles compare with that described by Ellis, as far as clefin content is concerned?

A. As I recall it, they are quite a bit higher than the product of Ellis.

Q. In the same general range, however?

A. Well, I will have to look it up. I don't see that he gives any iodine numbers.

Q. He expresses it in percentages of olefins?

A. Well, his percentages of olefins I don't know just how to compare with Ellis' product.

Q. You don't know the percentage of olefins in the Ellis product?

A. No, I do not—I know the general order of magnitude. [fol. 1229] Q. You say you have a general idea of the quantity of olefins in the Ellis product?

A. Yes.

Q. About what is that?

A. Somewhere in the range of 20 to 40 per cent., I should say.

Q. It averages about one-third of the total quantity of hydrocarbons, does it?

A. I am not talking about the average.

Q. It might be one-third?

A. It might be one-third, or it might be something else.

Mr. Rauber: I am asking about the product described in "Exhibit No. 98," how it compares as to olefin content, with that described in the Ellis patent.

The Witness: Apparently the oil that he is referring to was obtained from heavy lignite oil, from heavy distillates, and I confess that I don't know what unsaturation there would be there. I would think that would be a good bit higher than Ellis, and I don't see——

Q. I want to call your attention to page 1296, to the quantities of bromine absorbed, and the proportions of unsaturateds—the lowest third of the page, there.

A. He says, there, that that corresponds to 44 per cent. anylene in one cut, and corresponds to 41 per cent. hexylene in another cut, and corresponds to 43 per cent. heptalene in another cut—but I will point out, first, that those are very narrow boiling ranges, cut out from the heart of the distillate, and do not give any evidence of what the percentage of unsaturateds might be in his product. He has concentrated on a certain narrow cut, and he says that corresponds to certain percentages. Now, that correspondence, we, as chemists know, is based on certain assumptions which, now, at least, we know are not correct. Normally we assume that bromine adds only onto double bonds on truly unsaturated hydrocarbons. If that were true that would mean that those materials would run 41 to 44 per cent. unsaturateds, so far as the narrow cut is concerned.

But that doesn't say how much unsaturateds you might get in the main body of the distillate.

[fol. 1230] Did you calculate the bromine numbers from the date given you at page 1296?

A. No, I did not.

Q. Roughly.

A. Do you wish me to?

Q. Just tell me whether it is over 90 or——

A. (Interposing.) Well, the first bromine number is around 100 and the second one is around 85, and the third one is a little lower.

Q. Iodine numbers for the same oils usually run considerably higher than bromine numbers, do they not?

A. Well, they usually run higher. Not always, but usually.

Q. Did you ever know of any running lower?

A. Yes.

Q. What?

A. Plaintiff's expert cited some cases of his own determinations.

Q. For unsaturated?

A. Where the iodine numbers on a given product ran lower than the bromine numbers. There are statements in the literature about that, too.

Q. Would it ever run very much below?

A. I note, for example, in a quotation from article by Johanson in "Industrial Engineering Chemistry" of 1922, wide variations. Refined paraffin wax, for example—which is, of course, saturated—gave a bromine value of 5.6 and an iodine value of 1.11. In other words, the bromine value was five times the iodine value. I do not say that is typical, but it shows what enormous variations you get, and you cannot calculate with accuracy bromine numbers from iodine numbers.

With regard to the plaintiff's expert's results there is one case—that happens to be on benzol—where the Allen 1881 bromine number was 1.96 and the Huebl iodine number was .6—in other words, less than a third as great. Of [fol. 1231] course, I do not mean to indicate that is the general rule, but it frequently happens that the iodine number is smaller than the bromine number.

Q. Frequently?

A. Yes.

Q. With unsaturated, or using it as a test for unsaturated hydrocarbon?

A. Well, it depends on what you mean by "when using it as a test for unsaturated hydrocarbons." That is what it is used for in these cases.

Q. How does the Ellis product compare as to olefin content and for bromine and iodine number with the product described on page 1424 under the head of "Shale Gasolines"?

A. You are speaking of the first line in the table?

Q. Yes.

Mr. Wiles: Are all those given in that table?

A. That has a bromine number of 62.2. It says nothing about the iodine number.

Q. And you have no idea how they would actually compare and test it?

A. I have an idea where it would probably be.

Q. Would a product starting in with the lowest initial obtainable in a cracking distillation, and having an end point of 125 degrees centigrade—would that be a gasoline?

A. Not if you took the lowest initial obtainable. That is assuming that you condense at very low temperature and under pressure. It certainly would not be a satisfactory gasoline.

Q. Suppose you condensed under the ordinary conditions and got an end point of 125, would that be a commercial gasoline?

A. I will have to have a little more data on that. Are you talking about operating this bulk pressure distillation apparatus on petroleum?

Q. Yes. You get a crude distillate, do you not?

A. You get a crude distillate.

Q. Now, you distill everything and take off a fraction, redistill it, and that fraction has an end point of 125 degrees centigrade, would that be a gasoline?

A. That would depend on the proportions of the other constituents that might be present, but if you followed ordinary plant practices, which would eliminate the very lightest constituents but keep in—no, I do not believe, if you followed the ordinary plant practices on that low a

cut, that it would be a satisfactory gasoline for summer use, because it would contain too much light ends.

Q. For the ordinary use?

A. Well, I have pointed out that gasoline, to be a satisfactory gasoline, must operate almost regardless of weather conditions.

Q. Suppose, instead of having an end point of 125 degrees, we make that cut higher, say at least for an end point of 150 degrees, would that be a gasoline?

A. It is not so much the end point I am worrying about, you understand; it is how much of the light ends. You must have enough of the heavy ends to carry the amount of light ends, which you normally produce, without getting a wild product.

Q. Now, suppose you have an end point of 150 degrees centigrade, could that possibly be a gasoline with a suitable initial point?

A. That could possibly be a gasoline; that might possibly be a satisfactory gasoline.

The Master: You mean if it did have a suitable initial boiling point?

[fol. 1232] The Witness: Yes, if it had a suitable initial boiling point and suitable proportions.

The Master: Which could not be much below 150.

The Witness: No, the initial boiling point should not be much below 100 degrees Fahrenheit.

Q. Then it would run from 100 degrees Fahrenheit to 150 degrees centigrade, which would be 302 degrees Fahrenheit?

A. If you had a product which boiled from say 100 degrees Fahrenheit to 302 degrees Fahrenheit and which had a substantial quantity boiling below 221 degrees Fahrenheit, say at least twenty per cent and preferably more, then, so far as the volatility is concerned, I should say that would be a satisfactory gasoline.

Q. Well, if it had an initial of a hundred and an end point of 302, which corresponds to 150 degrees centigrade, it would ordinarily have a proper distribution, would it not?

A. It would ordinarily, yes. Not necessarily, however. You could make up mixtures which would fulfil the first two conditions which would not fulfill the third.

Q. What sort of a mixture would that be?

A. Materials that have a little bit of light ends and then all the rest crowded in between 250 and 300.

Q. Suppose you distilled in an ordinary laboratory apparatus, placing the thermometer in the ordinary position, not using steam and at atmospheric pressure made your cut at 125 centigrate, would the material at that temperature be a suitable gasoline?

A. We have two ordinary places that we put thermometers in ordinary work; one is in the liquid, one is in the vapor; that is what I want to settle.

Q. In the vapor.

A. Putting the thermometer in the vapor?

Q. Yes.

A. Again, it would depend largely on the proportion of light ends, whether you had too much below 100 or not enough below 221.

Q. Assuming that it were the condensate from a tube cracking still, such as that of the Ellis Patent 1,318,060?

A. Well, it would be an easy matter to make material from that which would meet those tests.

Q. That would be a gasoline, you mean?

A. That would have the proper volatility.

Mr. Rauber: Read my question.

(Last two or three questions and answers repeated by the stenographer.)

Q. Proper volatility for a gasoline?

A. Yes.

Q. That is, taking everything from your distillate that runs up to 125, and making your cut, with a thermometer position as you have described, the condensate would be a suitable gasoline?

A. Not necessarily.

Q. It would not?

A. I stated definitely what it is; that it would be an easy enough matter to make a satisfactory product from that, but I would not say that every product which you might make under those conditions would be satisfactory.

Q. How would you make a satisfactory product under those conditions from that material?

A. I would watch the distillation curve, prevent it from having too much light and yet be sure I had enough boiling below 221. If I met those two conditions I would be satisfied it would be a suitable gasoline.

[fol. 1233] Q. Then, by merely making your cut below 125, you would not necessarily make a suitable gasoline?

A. Not necessarily, no.

Q. If you carried the same operation through and made your cut at 150, would you get a suitable gasoline?

A. Not necessarily. Probably you would but not necessarily.

Q. If you cut at 105 degrees centigrade, you would not have a suitable gasoline either, would you, under those same conditions?

A. Not necessarily. You might be able to make a suitable gasoline.

Q. How would you do it?

A. I would be sure that my initial was not too low; and be sure that I had enough boiling below 221.

Q. How high would you have to have your initial?

A. It ought to be above—well, with that much light ends I would like to see it with an initial point above 100.

Q. Above 100 degrees Fahrenheit?

A. Yes.

Q. And if you cut at 105 degrees centigrade, which would be 221 Fahrenheit, you could obtain a suitable gasoline?

A. I do not see why it would not operate perfectly satisfactorily in a car; therefore, I would consider it a suitable gasoline. Of course, no one would want to make as light a gasoline as that.

Q. It would not be suitable to sell in the ordinary filling stations?

A. I don't think people would kick at it. I mean, assuming it is properly refined in other ways and meets other specifications. As far as the volatility is concerned, I should think, if you did not have too much light ends——

Q. That is, if you had an initial of over 100 Fahrenheit?

A. Had an initial of over 100.

Q. Have you any idea where the end point would be?

A. Of course, I must say that we are getting out of the range of the things I testified about. I am departing a good bit into what I would expect from theory.

Q. You have been laying down some requirements for a gasoline and I have been wondering if these requirements we have been talking about would meet the requirements?

A. When you get so far away from the ordinary conditions, I cannot be sure, without testing it, whether it would or not. There is also some indication that you must not [fol. 1234] have a more than a certain amount below 160, and tests have been made, which seem to indicate that.

Q. On page 1926 of the exhibits, lines 87 and 88, Ellis mentions sulphuric acid as having a content of 70 and 80 per cent of sulphuric anhydride.

A. 1926?

Q. 1926. Do you find that?

A. Yes.

Q. To what percentages of sulphuric acid would those two figures correspond?

A. Well, if he means sulphuric anhydride?

Q. That is what he says, is it not?

A. In the sense in which we use it today, at least, that would be quite stronge acid of course.

The Master: How strong?

A. That would be on that assumption, varying between about 86 and 98 per cent sulphuric acid.

Q. 98 per cent sulphuric acid is stronger than is usually used in refining petroleum products, is it not?

A. Yes, it is, distinctly so. It would not leave any appreciable amount of olefins unattacked.

Q: You mean that a small amount of such acid would take all of the olefins out, practically all of them out, without regard to how much olefins were present?

A. Oh, if you use a very little bit you might not—you would not take them all out.

Q. But if you used any considerable quantity?

A. If you used any considerable quantity you would go after your olefins very rapidly, yes.

Q. In the ordinary olefins, those that do not resinify, the sulphuric acid unites with the olefin hydrocarbon molecule for molecule; is that right?

A. I do not think it is, but it might be. A great deal of what happens seems to be polymerization into gummy compounds, which I imagine are quite separate from the acid.

[fol. 1235] In other words, the acid may act more as a catalytic agent instead of reacting molecule for molecule.

Q. There are only two ways it would react, are there not, one is to resinify and the other to combine with the unsaturated; is there any other way the sulphuric acid can affect them?

A. Well, you can polymerize without necessarily resinifying.

Q. If you polymerize don't you form resins?

A. Possibly, but not always. The higher polymers are resins and the lower polymers are not resins.

Q. How would you classify them?

A. Well, they are polymers.

Q. Are they sticky?

A. No, they stay dissolved in the oil as a rule.

Q. In regard to the Palmer patent, this patent is for digestion without distillation, as I understand it; is that correct?

A. Yes. That is the novel idea in Palmer, that it is possible to bring about cracking by digestion in the absence of distillation.

Q. Can you give me the dimensions of the coil in the Cross process?

A. Can I?

Q. As actually used.

A. No, I cannot. They are in the same general order of magnitude as the Holmes-Manl-y coils, I know from seeing them; somewhere between two and a half and three and a half inches inside diameter. I cannot give it to you exactly.

Q. Do you know the rate of feed to the Cross unit?

A. I have that some place, but I do not have it with me. I will look it up.

Q. All right, I will just get it later, then. Referring to the Cross process illustrated in Exhibit 412, in what phase are the hydrocarbons in that reaction chamber?

A. As I have stated, they are both in the vapor and the liquid phase.

Q. In the discussion of the Adams process you listed among the seven important features disclosed by Adams the application of heat to the intermediate portion of a vertical cracking still. Do you find this feature in Adams patent No. 1462677?

A. I judge that the oil is sprayed in and flows down. I would have to read through the operations to make sure there never was any liquid level carried. That was what I was particularly concerned about.

The Master: That answer, where you refer to the liquid level, referred to another patent of Adams?

The Witness: Yes.

The Master: Where the liquid level was marked on the patent drawing.

The Witness: Yes, the one we referred to the other day.

Q. Then your idea is that this patent does not disclose that feature?

A. I would have to read through the disclosure, but if the showing of the spray there represents essentially the method of operation, that is certainly not a bulk pressure distillation and this is not a cracking still fired below the liquid level, but I would have to read through to make sure that is the only operation considered in this patent, if you desire.

The Master: Would this be more analogous to a flash cracking?

The Witness: Yes, decidedly so, your Honor.

Q. I will also call your attention to patent No. 1462678. [fol. 1236] Do you find this feature of the application of heat to the intermediate portion of a vertical cracking still below the liquid level present in this patent?

A. Well, this seems to be apparatus for the same process, and so I should say the same answer applied.

Q. That is, you do not find it?

A. Well, I would have to read clear through the disclosure to be sure whether it is there or not. If you wish me to do it I will be glad to. If he only sprays in oil as shown in the figure, then he certainly does not have it.

Q. Do you understand this Adams patent No. 1462677 to be a distillation process for making gasoline?

A. What do you mean by "distillation"? Do you mean bulk pressure distillation?

Q. Leaving out the distillation, would it be a process for making gasoline?

A. I think so.

Q. Do you think you could successfully make gasoline by the process shown in this Adams' patent?

A. Just what do you mean by successfully?

Q. Could you make it commercially?

A. At a profit?

[fol. 1237] Q. Yes—you have used the word "commercially," haven't you?

A. I have used it in connection with processes that were commercial, and where I knew it was possible.

Mr. Royce: Where you knew it was profitable?

The Witness: Where I knew it was possible to make a commercial product, but to say, from a patent disclosure, whether you can make, with it, gasoline at a profit, commercially, would take a month to go over it and report on it, and you would have to figure on it, to make it accurate—you have to take into consideration things that are not apparent on a casual inspection.

Q. You have only given these Adams patents a casual inspection, then?

A. I have studied some of them more closely than others, of course, but I have not attempted to figure out the cost of operation of the different parts of the apparatus, or the cost of the apparatus, or their capacities. It would certainly operate to make gasoline. There is no question about that—but when you ask if it will make gasoline at a profit, that is a thing that I would have to look into pretty thoroughly.

The Master: Have you ever seen a still constructed like this Adams patent, No. 1462678?

The Witness: No, I have not. I have seen stills that had certain of the features, undoubtedly, but not the combination as described there—I haven't seen that.

Q. Do you know whether you could make gasoline by the process described in the patent to Intschick, "Exhibit No. 120"?

A. Well, that is a gas making process, and I presume, like most gas making processes, there is a certain amount of volatile liquid which, by very heavy and expensive refining processes might be made into a satisfactory gasoline. I would not consider it to be a gasoline making process. It is a gas process, essentially.

Q. Does he say it is a gas making process?

A. Yes.

Q. You make gas, also, in gasoline making processes, don't you?

A. Yes, but we don't call them gas making processes.

Q. It is merely a question of what you call a thing, which determines what you are going to make?

A. No, not merely a question, but generally the name is fairly accurate.

[fol. 1238] Q. In Adams' patent No. 1327263, I believe you identified the tube, 12, as being the indirect heating element —it is on page 2016 of the Exhibit Book?

A. Yes. What was the question?

Q. You identified as being the indirect heating element the tube 12, did you not?

A. That is certainly an indirect heating element, but there is, in the methods of operation, an additional pre-heating element.

Q. But you would consider this tube, 12, the main indirect heating element, would you?

A. Well, I don't know whether you would call it the main one, or not. He says, with regard to the other heating element, on page 2022—that is, I am speaking of the pre-heating element, now, which the oil passes through before it goes to that heating element—Now, reading from page 2018, line 102, he says: "It is best to heat the oil before it is admitted to the converter, B, so that the heat from the furnace of the converter may be conserved for the more important function of cracking, splitting the molecules of and rapidly vaporizing the oil, and, therefore, the oil heater H is provided, in which the oil may be brought to any desired temperature within the container 5, by gas blast from jets, 6, or by other economical means of heating".

So that I should consider that that would be at least in certain methods of operation, the very important, and possibly the most important part of the heating coil.

Q. You think most of the heating is done in 5?

A. It certainly must be.

Q. What do you think Adams intended to disclose?

A. Well, I just read exactly what he disclosed—that he wants to conserve the other heat for cracking, and, therefore, he provides the pre-heater, H, in which the oil may be brought to any desired temperature by an economical means

of heating. In other words, I judge that he wanted to do as much heating as he could, in this coil, up to the point of beginning to crack.

Q. And then he would put considerable more in the vertical tube, 12?

A. Quite a little more, yes.

Q. Does this patent also disclose the application of heat to the intermediate portion of a vertical cracking still?

A. Yes.

[fol. 1239] Q. Then that corresponds to the tube 12, does it?

A. Yes. From that standpoint the tube 12 corresponds to a vertical still which is heated below its liquid level, the oil having been previously heated substantially to a cracking temperature in a pre-heating coil.

Q. Mr. Wilson, on page 5723 you have listed seven important features of bulk pressure distillation first disclosed by Adams.

A. Yes.

Q. That is page 5723 of the typewritten record. Do you mean that each one of these features is shown in every Adams patent?

A. Oh, no; decidedly not.

Q. Can you point out just which patents contain each feature?

A. That will take a little time.

Q. Then, if it is going to take considerable time I would suggest that you do it over night. Would you mind doing that?

A. All right. Do you want me to give—I suppose you will be content with one patent which shows each feature? I do not need to enumerate every patent that shows a given feature?

Q. No, what I would like is, take for instance, A; point out where you find this feature and the elements showing it.

A. Yes. I might say that in discussing these features I have not closely distinguished between apparatus and process. As a rule, Adams has a pair of patents on the given features; the process of carrying it out and the apparatus for carrying it out.

Q. Yes.

A. The indirect heating of a bulk supply of oil, I have

already pointed out, is in patent No. 1,327,263 and in patent No. 1,320,354.

The feeding of fresh oil and drawing pressure tar continuously and apparatus for same are disclosed in these same two patents.

The application of heat to the intermediate portion of a vertical cracking still below the liquid level is disclosed in patent No. 1,327,263.

The use of agitators and carbon scrapers in the heated portion of a vertical cracking still and drawing off the tar and scrapings from the bottom is also disclosed in Patent No. 1,327,263.

The use of a plurality of vertical cracking stills operated in series with a (it should be "a common") vapor line is found in the patent No. 1,433,519.

The separate condensation and re-cracking of that part of the cracked vapors which are heavier than gasoline is shown in patent No. 1,320,354, for example.

As I say, those are by no means exclusive, because the same features are repeated, in divisions, and subdivisions.

Mr. Royce: How about the second on that page, the condensation of gasoline distillate and separation of gas therefrom at still pressure.

The Witness: Oh, I beg your pardon. That is shown, for example, in Figure 11 of Patent No. 1,320,354.

Q. Is it shown in any of the other figures?

A. Oh, it may be in very many.

Q. I mean in that one patent, could you tell me?

A. I do not think the means for accomplishing that is shown in the other drawings of that patent.

Q. During your testimony of October 14th you conducted an experiment here, having a hot incandescent wire immersed in a body of Nujol?

A. Yes.

Q. What was the diameter of that wire?

A. I think I stated it at that time; I do not recall now.

[fol. 1240] Q. It was less than a hundredth of an inch in diameter, was it not?

A. No, I should say it was just—No. 30 somebody says. It sounds familiar; No. 30 sounds familiar.

Q. Why didn't you use a carbon grid of the type shown in the Adams patent?

A. I did not consider using a carbon grid. I was simply trying to get some convenient method of showing that there was no necessary relationship between the temperature of a heating surface and the temperature of the surrounding material. I took the most convenient thing available which was a small wire. A carbon grid would, of course, have required very much more current to keep it red hot.

Q. And it would have been nearer the conditions as shown in the Adams patent, of course.

A. I was not trying to carry out the Adams process.

Q. No, but you were trying to show that in the Adams patent you would have liquid around that carbon grid; was that not the purpose?

A. No, I was simply trying to show that the statement of plaintiff's expert, that there was necessarily a fairly close relationship between the temperature of the flowing stream of oil, or body of oil, and the heating material was not correct in point of fact.

Q. He was speaking of the apparatus shown in Adams' patent, was he not?

A. Well, I suppose that was the general context, but he was asked, as I recall it, the specific question—a general question as to the relationship between the temperature of the heating surface and temperature of a flowing stream. I mentioned the page in the record where he made the statement in connection with my own testimony, or it was in the question more likely.

Of course, exactly the same thing can be done with a carbon electrode if you have enough current to accomplish it.

Q. You could put a carbon electrode in that beaker which you had and accomplish the same thing?

A. In my opinion you could. I have not actually tried it. I see no reason why not. It would take much more power than we could get from the storage battery, however.

Q. It would give a great deal more heat, too, would it not?

A. Oh, yes. The temperature of the oil would rise more rapidly than it did with the electrode, certainly.

[fol. 1241] Q. If you had that carbon grid occupying the greater portion of your space in your beaker and heated it white hot or even a dull cherry red, do you think that you would have accomplished much the same thing that you did with a little bit of a wire?

A. Are you talking now about going to something like the Adams' operation?

Q. Yes.

A. The important thing in Adams is that he did not have a stationary system but a flowing stream. Of course, with the flowing stream you can get any combination you want, depending on how fast the stream is flowing and how warm the oil is when it came in.

Q. You think he put that in there so fast that it would tear away any film?

A. Well, what kind of a film do you mean, a gas film?

Q. A film of vapor, yes.

A. I do not know whether he did or not. I imagine if it was red hot and he had fluid oil he may have had a film of vapor, but it may have been a film of coke.

Q. I understand you to say in regard to the correlation of temperature and pressure that that was not necessary in the Adams patent, because his cracking all took place around his heating element in a film of vapor.

A. Yes. It would be in general a vapor film if your electrodes were red hot. It would in general be a film of vapor.

The Witness: Oh, I did not make that statement about the cracking. I did not make that statement. I did not understand that you said all the cracking took place at that point. I did not say that.

Q. The greater part of it would, I suppose, would it not?

A. Of course, that would depend on the method of operation. You might carry out most of it there, or might carry out a comparatively small portion. But I also made clear, particularly in the case of the externally fired apparatus, that the insulating film might be a film of coke.

Q. But that your velocity was not sufficiently great to tear that film away?

A. A film of coke?

Q. A film of vapor.

A. I do not recall just what you are referring to.

[fol. 1242] Mr. Atwell says that that wire, as near as he can now find was a No. 30, which is .0125 inches. In other words, 12.5 one thousandths of an inch in diameter.

Q. You realize, of course, that the conditions under which you are conducting that experiment were entirely different from what Adams discussed in his patent?

A. Oh, undoubtedly. It just illustrated one simple fact.

Q. In the process of the Humphreys, Rogers and Paulus Patent you have in the space above the liquid some permanent gases?

A. It would be a very small percentage of the total in that type of process.

Q. You would have some gasoline vapors?

A. Yes.

Q. And some kerosene vapors?

A. Yes.

Q. Would that be true if both the temperatures and pressures were raised?

A. That could be true, but it would not necessarily be true. It depends on whether you kept exactly the proper correlation such as he describes.

Q. Could that correlation be carried up to a temperature of 850 degrees Fahrenheit; would it hold up to that temperature?

A. I do not know just what you meant by that.

Q. With the conditions set forth in that Humphreys, Rogers & Paulus Patent.

A. No, the highest temperature he sets forth is 810 in his patent.

Q. And you could not go above that temperature?

A. Well, that is for someone else to carry it up there.

Q. I am speaking of the disclosures in that process. It is understood it does not go above 810; is that correct?

A. It is clear that he gives figures up to 810. Now, a person might make a slight extrapolation beyond that. I do not think it has ever been done in bulk pressure distillation beyond that point.

Q. From your reading of that patent you cannot say whether you can carry that correlation up to 850 or not?

A. I presume there must be some correlation and some cracking rate which you could get by going to very high pressures, extremely high pressures, at temperatures up to 850, but I would not know exactly what they would be and what the cracking rates would be.

[fol. 1243] Q. Then you would still have in your vapor space permanent gases and gasoline?

A. Yes.

Q. Do you know what the critical temperature for gasoline is?

A. No.

Q. If you had a mixture of various hydrocarbons and knew their critical temperatures—the critical temperatures of the individual hydrocarbons—could you calculate the critical temperature of the mixture?

A. I could not do it on the stand. It might take——

Q. But it could be done?

A. I think that if you are dealing with what is termed ideal substances—those which obey certain thermodynamic rules—(which, in practice, we do not have) you could approximate pretty closely, if you knew their individual critical points.

Q. If you knew the critical temperature of the constituents of gasoline, you could calculate the critical point of the gasoline?

A. On certain assumptions, and by the aid of certain rules.

Q. Of course you would have to use rules to calculate?

A. I simply want to make it clear that it is more like calculating iodine numbers from bromine numbers, than it is like calculating Baume gravities from specific gravities. In other words, it is not an open and shut proposition, but you could probably approximate, if you knew the critical point of all the constituents.

Q. In the tube and tank process do you hold the pressure in your digesting, or reaction chamber so high as to prevent any substantial vaporization of the oil?

A. No, I think you get quite a little vapor formation—certainly a good bit of the gasoline formed would tend to go into vapor form.

Q. Would it amount to 50 per cent?

A. 50 per cent of what?

Q. Of the material put in—would that much be vaporized?

A. No, I shouldn't think so.

Q. About how much would you estimate?

A. Well, you are in a range where it is pretty hard to tell. You have so many different things present. If you just had the gasoline hydrocarbons, alone, you would have one thing, but you have this heavy pressure tar, which has a dissolving action.

[fol. 1244] Q. In other words, there is a substantial vaporization, but you don't know how much?

A. There is a substantial vaporization, but I don't know how much.

Q. Do you know whether any of the cracked gasoline sold around New York contains gum, or resinous materials?

A. Well, if it is like the cracked gasoline around Chicago, some of it does.

Q. And it would be large in olefins, if it were cracked material, would it not?

A. Unless it had been treated very heavily, which is not ordinarily done, today.

Q. The product made by the Burton stills, was, of course, sold before the Ellis patent issued, wasn't it?

A. When did that issue?

Q. I think around 1919, or some such date.

A. Yes, it undoubtedly was.

Q. Now, in discussing the cracking or kerosene in the ordinary distillation, you said that if you blew in an inert gas you did not get any carbon on the inside of your distilling vessel. Is that correct?

A. No, that is not correct.

Q. On page 6165 of the Typewritten Transcript, I find this sentence: "The one which has the oxygen free gas going through it, to keep oxygen away from the kerosene, will boil for hours without appreciable change in color."

A. Yes.

Q. That referred to the blowing of the inert gas into your distilling vessel?

A. No, that was more in keeping an atmosphere of inert gas in the condenser above the distilling vessel. This was a reflux condenser that I was speaking of.

Q. What part of the apparatus was the inert gas blown into?

A. As I recall it, we introduced it in a jet, going down into the reflux condenser, so that it would soon sweep out the air from the system. We probably also drew some air through the flask at the start, to get out the inert gas, and then we kept a small stream going into the condenser, so that no air could diffuse back and get into contact with the kerosene.

Mr. Wiles: I will ask the Reporter to just read that answer, and see whether you said what you wanted to say, Mr. Wilson.

[fol. 1245] (The Reporter read, as follows:

"Answer. No, that was more in keeping the atmosphere of inert gas in the condenser above the distilling vessel. This was a reflux condenser that I was speaking of.

Question. What part of the apparatus was the inert gas blown into?

Answer. As I recall it, we introduced it in a jet going down into the reflux condenser, so that it would soon sweep out the air from the system. We probably also drew some air through the flask"——

Mr. Wilson: No, I should have said "We probably also drew some inert gas through the flask."

(The Reporter continued reading: We probably also drew some inert gas through the flask at the start, to get out the inert gas"——

Mr. Wilson: No, that should be "air."

(The Reporter continued reading:

"We probably also drew some inert gas through the flask at the start, to get out the air, and then we kept a small stream going through the condenser, so that no air could diffuse back and get into contact with the kerosene.")

Q. Now, in the prior art, the patent to Young, was the body of oil maintained at a cracking temperature sufficient to give——

A. Exhibit number?

Q. The old British patent, Exhibit 93. In the Young patent of Exhibit 93 was a body of oil maintained at a cracking temperature sufficient to give a substantial increase in the boiling point?

A. A substantial increase in the boiling point?

Q. Of the oil, or decrease in the boiling point of the oil?

A. I should think so, yes.

Q. That is true of Dewar & Redwood?

A. Yes. At least, if by decrease in the boiling point, you mean——

Q. The lowering——.

A. (continued). The formation of products which have a lower boiling point than anything in the stock.

Q. Yes.

A. Of course, the average boiling point might not decrease, because you have some things going in the other direction, but you certainly do form lower boiling products.

Q. That is true of Riebeck and of Barbet?

A. Yes.

[fol. 1246] Q. And the evolved vapors are continuously removed from a point above the liquid level?

A. In some cases where you have a loaded valve, that removal would, of course, be more or less intermittent.

Q. But it is removed above the liquid level?

A. When it is removed it is removed above the liquid level, yes.

Q. And they are all operated under pressure up to that point? Under pressure above atmospheric?

A. In the still?

Q. Yes.

A. Those that you have mentioned are.

Q. Do any of the patents of the defendants in this case use a siphon or jet condenser?

A. Any of the processes?

Q. Any of the patents which you are familiar with use a siphon or jet condenser for condensing their condensate?

A. I do not recall any just now, but that is a very broad question for the number of patents involved.

Q. Have you ever had charge of a commercial Burton cracking unit?

A. Why, not in the sense of being the stillman, no. I have directed experiments on a commercial sized unit and been responsible for laying down the conditions under which it should operate, but of course, a stillman is the boss on the job when it comes to actually running the still.

Q. There is someone else in charge of the operations, though, is there not?

A. I am not the refinery superintendent or anything of that kind, if that is what you mean.

Q. Did you ever design a Cross commercial sized unit? cracking unit on a commercial scale?

A. No.

Q. Did you ever design a commercial Cross cracking unit?

A. No.

Q. Did you ever help operate a Cross commercial sized cracking unit?

A. No.

Q. Did you ever design a Cross commercial sized unit?

A. No.

Q. Did you ever have charge of the operation of a commercial tube and tank process unit?

A. No.

Q. Did you ever design a commercial tube and tank unit?

[fol. 1247] A. No.

Q. Did you ever help to operate a commercial tube and tank unit?

A. Yes.

Q. Where?

A. At the Bayway plant of the Standard Oil Company of New Jersey.

Q. When?

A. Oh, two or three years ago, I think. I have forgotten just exactly when; about three years ago I guess.

Q. How long did you continue to do that?

A. Well, I was working there on a special test for about ten days, I guess.

Q. And you helped to operate a commercial sized tube and tank unit?

A. In the sense of making suggestions to do this and that, changing the operating conditions and studying just what happened.

Q. Did you ever have charge of the operation of a commercial Holmes-Manley unit?

A. Only in the same sense I mentioned in connection with Burton, of supervising tests on it.

Q. You were just making observations?

A. Well, there is a good deal more involved in running a test than just making observations. That is, we would lay out a schedule for just how the test was to be conducted, just what conditions we were to try to operate under; then we would decide in the course of the test to change certain conditions and we would arrange with the stillman to have those conditions changed, so in that way I was responsible, in important respects, for the operation, although not in the same way that the refinery superintendent is.

Q. You never operated an Adams process unit, did you?

A. Just what do you mean by "An Adams process unit?"

Q. Anything shown in any of his patents?

A. Well, I have operated many stills that had features shown in his patent, certainly.

The Master: Had the same features?

The Witness: Had the same features, yes.

The Master: Incidentally, or embodied——

The Witness: Embodied in the other types.

The Master: You never operated a straight Adams process?

[fol. 1248] The Witness: I never operated exactly the patent drawing process, no.

The Master: Did anybody else that you know of?

The Witness: Yes, I think there is one operating today, as a matter of fact, practically a duplicate.

Q. Of the Adams?

A. Yes.

Q. Where is it operated?

A. Tulsa, Oklahoma.

Q. By whom is it operated?

A. The Texas Company.

Q. You know what size unit it has?

A. No, I do not know the full details. It is not a full scale commercial unit, but it is large enough to do very substantial cracking.

Q. To which one of the patents does it correspond?

A. The process patent.

Q. Do you know which number that is, the early one?

A. Excuse me. The patent number is 1,327,263.

Q. Now, there is only one other thing I would like to ask you to bring in, and that is the rates of feed; bring that in tomorrow morning.

A. For Cross?

Q. Yes, and the size of the digesting or reacting chamber.

A. Of Cross?

Q. Of Cross.

A. All right.

Q. And also the same for tube and tank and Holmes-Manley?

A. I have given that for tube and tank and Holmes-Manley in my direct testimony.

Q. The size of the coils?

A. Yes.

Q. The rate of feed?
A. The rate of feed.
Q. The size of the battery?
A. And the size of the drums.
Q. That is just for the Cross, then.
A. The Cross; all right.

Mr. Royce: Mr. Wilson, while Mr. Rauber is checking [fol. 1249] over his notes, I would like to ask you one or two definitions of terms which I take it are familiar and have been for the last four or five years in the oil refining-cracking industry. If they are not terms that have been familiar during that period, please say so. If they have been and you know the definitions that are generally accepted, why, give them.

By Mr. Royce:

Q. The first is shale oil.
A. Shale oil is commonly considered as being the product of the distillation of oil shale at temperatures sufficient to decompose the organic material which it contains.
Q. Petroleum oil.
A. Petroleum oil is a naturally occurring liquid, flowing or pumped from the ground, and consisting mainly of hydrocarbons with small amounts of sulphur compounds generally, and occasionally oxygen and nitrogen compounds in still smaller quantity.
Q. Petroleum?
A. That is just what I just defined.
Q. I asked you petroleum oils. Is there a difference?
A. The definition I gave——
Q. Does that answer stand for petroleum?
A. Yes, that answer stands for petroleum.
Q. Now, I will ask for petroleum oils.
A. I should say a petroleum oil was any liquid product derived from petroleum. It is true that sometimes the lighter products are not invariably considered oil. Sometimes, when you speak of oils you mean the heavier products, but ordinarily a petroleum oil is any product which is liquid at ordinary temperatures, which is derived from petroleum.
Q. Now, as to any one or all of these three terms, would they be considered in a different sense abroad? They are American terms, of course?

A. I have given their definitions in the American sense.

Q. You have given their definitions in the American sense?

A. Yes.

Q. What about motor spirit; what does that mean?

A. Well, the general usage of the term "motor spirit" in this country is to name something they cannot call gasoline, because it differs in some particular way from conventional gasoline, at least. It may be better, it may be worse. On the other hand, motor spirit is, even in this country, sometimes used as a synonym for gasoline.

[fol. 1250] Q. In England I think they use motor spirit the way we use gasoline, is that correct?

A. Yes, in England I think their use is practically synonymous but it is not generally so used in this country.

Q. What would the initials "D S" gasoline mean?

A. That is deodorized stove gasoline, probably.

Q. Did you earlier give your age, Mr. Wilson?

A. I do not believe so.

Q. What is it, please?

A. I am thirty-three and a half.

Q. And I take it you have been at these hearings since the start of the case?

A. I have been at some of them.

Q. Along with counsel for the defense?

A. Yes.

Q. And through Dr. McKee's testimony were you present?

A. I was present through Dr. McKee's testimony, yes.

Mr. Rauber: There is just one other question I would like to ask before I close.

By Mr. Rauber:

Q. In connection with the Pielsticker patent, you mentioned the presence of salt water in the oil. Did you ever find that in any of the mid-continent crude or Gulf crude?

A. Oh, yes, practically any crude oil.

Q. You never find it in shale, do you; shale oil?

A. You probably would not find salt water.

Q. Emulsified water?

A. Oh, yes, you would be very likely to find emulsified water.

Q. In the distilled crude oil?

A. Yes, because one of the products of decomposition of oil shale is frequently water.

Q. You do not think there has been any distinction made by European writers between petroleum and shale oil?

A. Oh, I would not say they never made any distinction. As I understand it, their common use of the term "petroleum" or "rock oil" was to designate shale oil, because that was the first rock oil known, and as I believe plaintiff's expert indicated, the Americans sort of stole that word for their new product.

The Master: You mean they applied it to well petroleum?

The Witness: Yes.

Mr. Wiles: Exclusively, as he said.

[fol. 1251] The Witness: We never called shale oil by the name "petroleum" here.

Q. But if in a foreign textbook you find shale oil under one chapter and petroleum under another, you would recognize petroleum there as being what we call petroleum?

A. I should judge that you might from the context reach that conclusion.

The Master: Mr. Wilson, as doubtless has been stated before in the Record, but if it has I am not certain that I have a distinct recollection of it—referring to Burton patent No. 1049667 on page 1, line 39 of the specification, it is stated: "A known method of treating the fuel and gas oils forming the aforesaid residue of distillation of the paraffin series of petroleum, for obtaining therefrom a low boiling point product involved" and so forth.

Can you tell me the precise disclosure referred to by Burton in that paragraph?

The Witness: As I mentioned, I feel sure that Burton must have been referring to the processes, such as Noad and Testelin & Rennard.

The Master: But he says, "A known method."

The Witness: Well, Testelin & Rennard would be the one that would come closest, because it has the high temperatures—what he calls destructive distillation though it is not bulk pressure distillation, the loaded valve, and handles a stock quite similar to that of Burton—that is, his stock was old in tube cracking processes.

The Master: You think that that description of the prior known method most clearly described Testelin & Rennard?

The Witness: Testelin & Rennard have essentially a tube cracking process, but they do refer to their enlarged tube, which is subjected to the hottest fire, as a still.

Of course they have no body of liquid in there. They have the characteristic tube cracking conditions, and they have the loaded valve and condensation at atmospheric pressure, and their product is an unsaturated product, characteristic of these high temperature tube cracking processes.

As I say, Burton implies, in his patent, that all of his gain in quality of product, over Testelin & Rennard, is due to condensation under pressure, in which he was mistaken. That is only responsible for part of his improvement, and the other part of the improvement is due to the fact that he is, in a bulk pressure process, cracking at a much lower temperature.

The Master: In other words, you think that there were other things involved besides those which he pointed out in his specification as the result of his method?

The Witness: I judge that Burton had tried out processes like Testelin & Rennard, and had tried out the other, and he describes clearly what his process is, and that process had several differences from Testelin & Rennard, and he attributed all his improvement in the quality of product, to one of those differences, whereas only part of his gain was due to that difference, and the larger proportion of his gain was due to the other differences.

The Master: But isn't it true that from line 69 to line 79, on page 1 of the patent, he limits the cause of what results he obtained, stating that "I accomplished this object"—viz: "This object I accomplished by raising the boiling [fol. 1252] point of the liquid residue and increasing the heat influence thereon, while undergoing distillation, by maintaining back pressure on said liquid, of the vapors arising therefrom by distillation, as hitherto has been done, as aforesaid, and also maintaining the vapors, themselves, under pressure, throughout their course from the still through the condenser, while undergoing condensation."

In other words, doesn't he expressly limit his step of improvement to the maintaining the vapors, themselves,

under pressure, throughout their course from the still through the condenser while undergoing condensation?

The Witness: He distinctly states that the improvement that he gets by his process is, in his opinion, due entirely to condensation under pressure.

But I don't see that that limits what his invention really was, because he described a process which did differ in other respects, which he apparently considered entirely immaterial at that time, but which we now know were very important, and that if you follow out his process you get results that could not have been gotten by these earlier processes.

He erroneously attributes the whole of this improvement in product, to one of the factors which made that improvement, but he does describe a process which had much broaded novelty than that one step.

The Master: By novelty you mean the correlation of heat and pressure of certain degrees?

The Witness: And the use of a material which was clean enough to crack in a bulk pressure distillation process, and yet maintain a black bottom in a commercial sized still.

The Master: I think I understand you, now.

The Witness: The feed rate of the Cross stills, of course, varies from unit to unit. They have two standard units at the present time, one with a thousand barrels a day, and one with two thousand barrels a day, which are the nominal capacities, at present, of the units. They will be somewhat [fol. 1253] greater for some kinds of feed stocks, and somewhat lower for other kinds.

The drum in the two thousand barrel unit——

Mr. Rauber: That is the reaction chamber?

The Witness: The reaction chamber in this unit is approximately forty-two feet long and fifty-three inches inside diameter. The pre-heater tubes are three inches inside diameter.

Mr. Rauber: And how long?

The Witness: Twenty feet long, and ninety-one tubes, I believe—approximately ninety-one tubes.

Mr. Rauber: Ninety-one times twenty would give the length.

The Witness: Yes, sir. Now, if you merely figure on those figures it gives you a somewhat higher ratio than

that which I mentioned—the ratio between the volume of the drum and the volume of the heating coils—than that which I gave yesterday, because that does not include the volume of the heating coils in the vapor heat exchanger, and that I was not able to get the figure for, for the two thousand barrel unit. But it is in the same order of magnitude. That is, the ratio of drum volume to total volume of material in the restricted passageway, where heat is picked up, is in the neighborhood of four to one.

Mr. Rauber: Did you give the size of the drums for both the two thousand and the one thousand barrel units?

The Witness: I don't believe I did give the sizes. I think the size of the drum for the one thousand barrel unit is forty-two inches inside diameter, and the same length approximately—that is, forty-two feet.

Mr. Cooper: When you speak of a thousand barrels, do you mean an hour, a day, or what?

The Witness: A day.

Mr. Rauber: Before closing, your Honor, I also want to offer in evidence the title pages, and pages 292 and 293 of "Outlines of Industrial Chemistry," by Frank H. Thorpe, 1899 edition, to which reference has been made in the cross-examination—as "Exhibit No. 471."

The said document, so offered, was admitted in evidence, was marked "Exhibit No. 471," and the same is herewith returned.

Mr. Rauber: I also offer in evidence the title page, and [fol. 1254] page 428 of the 1922 edition of Day's Handbook of Petroleum Industry, and ask that it be marked "Exhibit No. 472."

The said page of Handbook, so offered, was admitted and read in evidence, was marked "Exhibit No. 472," and the same is herewith returned.

Mr. Rauber: I also offer in evidence pages 566 and 567 of the American Petroleum Industry, Volume 2, by Bacon & Hamor, to both of which reference has been made in cross-examination—as "Exhibit No. 473."

The said pages, so offered, were admitted and read in evidence, were marked "Exhibit No. 473," and the same are herewith returned.

Redirect examination.

Questions by Mr. Wiles:

Q. In your direct testimony, and again, just now in answer to a question by the Master, you stated that when Burton, in his application, and in patent No. 1049667, alluded to the tube type of process, and the flash gas making processes, as being the only ones which used his, or like stocks in cracking——

The Master: Oh, I don't know—that is a pure inference.

Mr. Wiles: No, your Honor, that is summarizing the witness's testimony. I am bringing back his direct. I want to go into that.

Q. You also mentioned Testelin & Rennard and Noad as typical examples of such processes. Is there any other process in the art cited, which is another example of this old type of process, which he distinguished from in this way?

A. Yes, Kittle is such a type of process. It is essentially a tube cracking process, which started with gas oil, and I might just as well have added that to the other two which I mentioned as using his type of stock, which, together with the flash processes, are the only ones which used his type of stock.

The Master: You keep referring to his type of stock as being gas oil.

The Witness: Yes, sir.

The Master: His type of stock were residues which were fuel oil and gas oil.

The Witness: No, his stock was gas oil, which is also [fol. 1255] termed fuel oil, and is the only type of fuel oil that is produced by the coking method of distillation.

The Master: This is very distinctly stated, it seems to me. After referring to the methods of distilling from crude petroluem of the naphthas, he says "This leaves the illuminating oils and the lubricating oils and waxes and, as residues, fuel oil and gas oil."

The Witness: Boiling from 600 to 700. With boiling points ranging from about 600 to about 700 degrees Fahrenheit.

The Master: Yes.

Q. This is in the record but perhaps, to clear it in your Honor's mind, I should put a question or two.

Mr. Wilson, is it a fact, first, that this description of Burton presupposes some antecedent process that is happening to his stock immediately before he takes it?

A. Yes, he is referring to the coking, the ordinary coking method which was the commonest method of running crude oil, and which is the method which characteristically produces gasoline, kerosene, lubricating oils, and waxes, and as residue or remainder the gas oil which is essentially what is left over in the refinery after you have made your maximum yield of the other four products.

That material boils at an intermed ate boiling point range mostly between six and seven hundred and is sold as either gas oil or distillate fuel oil.

Q. Yes. In your direct you also stated that at the time Burton wrote there were only three processes regularly practiced in handling crude oil, the skimming process and the coking process and the running for steam refined.

Now, taking the first of those, the skimming process, does it produce lubricants and wax?

A. No, it does not. Within recent years, in some cases, the skimming operation on certain crudes has been continued to take off lubricants, particularly on Gulf Coast crudes; but even then they do not make wax.

The skimming process characteristically does not give the products mentioned here as produced by the antecedent operations. It does give a residual fuel oil, but that cannot be the type of fuel oil Burton is referring to; first, because it does not boil between six and seven hundred, and, second, it does not come as a product along with lubricants and wax.

Q. And the skimming process in the form in which it was [fol. 1256] used in those cases produced, as I have understood your testimony, gasoline, kerosene and reduced crudes, is that correct?

A. Yes.

Q. It did not produce lubricants and waxes?

A. No.

Q. Now, the second process you have just discussed, the coking process, that does produce gasoline, kerosene, lubricants and wax, does it not?

A. Yes.

Q. And its remainder or residue is the gas oil or distillate fuel oil?

A. Yes.

Q. Now, the third process, the running for steam refined, also produces, does it not, gasoline, kerosene, lubricants and wax?

A. Yes.

Q. And it also has the same residue of gas oil or distillate fuel oil?

A. Yes, the same material left over from the operation, not going into the other four major products.

Q. So that the only processes which answered to the antecedent description of Burton had as their residue gas oil or distillate fuel oil?

A. Yes, residue in the sense of remainder.

Q. Yes, remainder. The remainder after making in the only processes that did make gasoline, kerosene, lubricants and wax?

A. That remainder was gas oil or distillate fuel oil and that was the only fuel oil made by those processes.

Q. Yes.

A. The only process that makes residual fuel oil is the skimming process which does not make the lubricants and wax.

Q. Now, those terms, gas oil, or distillate fuel oil, were used synonymously and interchangeably, were they not?

A. Yes.

Q. The gas oil or distillate fuel oil answers exactly to the description Burton gives in his patent and it is the only refinery product having the disclosed properties of his stock, is it not?

A. Yes.

Q. And residual fuel oil or reduced crude produced by the skimming process does not answer to that physical description?

[fol. 1257] A. No, not at all. They would boil at much higher temperatures.

Q. Now, in your direct you mentioned the citation of this Testelin and Rennard patent in the Burton application. Was this identification of the patent as having been cited by the Patent Office correct?

A. No, I was wrong in thinking that the patent which was cited in the file wrapper was the same patent to Testelin and Rennard which has been cited in this case. It was a different patent which I had not seen, to these same patentees.

Mr. Wiles: The Testelin and Rennard German patent, which was cited by the Patent Office, is substantially the same as the Rennard British Patent, which appears in this record?

Mr. Rauber: The British patent was used rather than the German to save translation.

The Master: And is Exhibit Number what?

Mr. Rauber: Exhibit No. 231.

Q. Now, does this statement—I mean, does the fact that this was not cited in the Patent Office in any way affect your conclusion as to the type of process that Burton was attempting to distinguish from in this paragraph of the specification?

A. No, not at all. Of course, Burton was making his distinctions before there was any citation by the Patent Office. That fact does not change my conclusion that Burton must have been referring to that type of process, namely, the type of process as described in the Testelin & Rennard patent and the somewhat similar Kittle and Noad patents, which are already cited in the record.

The Master: That is, he was referring to them in that paragraph.

The Witness: Beginning line 39.

The Master: Beginning line 39: "A known method."

The Witness: Yes.

Q. Now, when Burton—while he was prosecuting his case, came to file an affidavit to show the practical accomplishments of his process, what kind of a process did he there distinguish from in that affidavit?

A. In this affidavit he distinguished from the hot tube type of cracking process, further indicating that that is the type of process he was considering that his was an improvement upon. In other words, he made his comparison with the only other type of process, except the flash-cracking [fol. 1258] processes, which used his type of stock, and he still apparently, when that affidavit was filed, believed that

his step of condensation under pressure was responsible for the entire difference instead of being responsible for only part of the difference between his results and that of others.

Q. Now, you said in your cross-examination that you thought gas oil might have been preferred for the making of gas in the flash process because it was cleaner and made less coke in these processes. Would the fact that it was known to be cleaner in these processes teach that it was clean enough to be cracked in a commercial Burton still to an extent sufficient to give present-day yields of gasoline without a hot bottom, or that it would crack at conversion rates at temperatures obtainable in a black bottom process?

A. You mean at commercial rates?

Q. Yes, at the commercial rates at temperatures obtainable in a black bottom process.

A. No, the mere fact that it was somewhat cleaner than other stocks as used for gas making, would not, in any sense, prove that it would be possible to crack it in a commercial sized bulk pressure still for the production of large quantities of gasoline without making a hot bottom.

As I pointed out, that type of still has a coke tolerance of around a quarter of one per cent. That is as much coke as it can handle, even with false bottoms which came later, and that was so much less coke than was given by any other known type of process, that no one could tell or predict in advance that gas oil would be clean enough for that purpose. And, in addition to that, there was absolutely no indication, so far as I can find, that gas oil could be cracked at commercial rates at the temperatures and pressures reasonably obtainable in bulk pressure distillation. It was generally known that it could not be cracked appreciably in the ordinary atmospheric cracking stills and it was cracked only at the very high temperatures characteristic of tube and flash cracking processes, higher than those obtainable in bulk pressure distillation at reasonable pressures for that day.

Q. And Thorpe & Young's publications also had something to say on that subject, did they not?

A. Thorpe & Young tried, as I pointed out, the material which of all those in the art is probably nearest to that of Burton, although of somewhat lower boiling point, and re-

[fol. 1259] ported it was not possible to crack it by repeated distillation back and forth under pressure.

Q. In view of your cross-examination on the subject, I want you again to contrast briefly the physical properties of Burton's gas oil and the oil that Riebeck called gas oil?

A. As I pointed out, Burton's gas oil was an intermediate distillate from petroleum, highly saturated, boiling mainly between six hundred and seven hundred degrees Fahrenheit, with a gravity in the neighborhood of thirty-two to thirty-four, and of course containing no lubricants.

Contrasting with this the material which Riebeck called gas oil, we see very large and very significant differences, the only similarity being in the fact that both were used for making gas, and that they therefore acquired the name of gas oil.

Riebeck's gas oil was, as I have previously indicated, a distillate from brown coal tar—the heavy part of the distillate from brown coal tar. That in itself shows it was made of entirely different types of compounds, including a great many oxygen compounds. Furthermore, he states that it can be used for making lubricants, which shows it is much heavier than Burton's gas oil; that its gravity given in Exhibit No. 97 was 23.6 and given in Exhibit 247, as 25.7, both in the lubricating oil range, at least for petroleum; both indicating, as I say, much heavier products than Burton's.

And he also points out in both of these publications that after taking off fifty per cent of distillate, the tar is a heavy black material heavier than water, which shows that it must be a very heavy, dirty stock, because Burton's tar would be nothing like that heavy, particularly after the relatively light amount of cracking to make kerosene.

It also was pointed out by Riebeck that when eighty per cent of decomposition distillate was drawn off the residue hardens enough to make a cokey asphalt. All of these indicate that the stock is different in sources and composition, but is undoubtedly a very dirty stock which could not have met Burton's conditions of cracking to make gasoline on a commercial scale without getting a red hot bottom.

Q. Without getting a red bottom?

A. Without getting a red hot bottom.

Q. I will ask you further, whether the Riebeck patent shows that he did get a red bottom.

A. Yes, the Riebeck patent speaks of the parts of the still [fol. 1260] being heated to a red glow, and in other publications, as I pointed out, he mentions the fact that twice, although he had a small still and a thick bottom, he split the bottom of his stills.

Q. As further bearing on the differences between these stocks, do we get any light from the change in the gravity-viscosity relationship which occurs on cracking?

A. Yes. As I pointed out in connection with the discussion on the twine oil patent, Burton starts with a material which has a gravity-viscosity relationship which is normal for petroleum oils, and as a result of this cracking operation his pressure tar has an abnormally low viscosity for its gravity; whereas, as I pointed out in the case of Riebeck's oil, cited by plaintiff's expert, his starting material has a relatively low viscosity for its gravity, though not as low as that of Burton's finished product, but the pressure tar he produced has a comparatively high viscosity for its gravity, showing a tendency to go in exactly the opposite direction during the cracking reaction from what Burton did.

This is in line, I believe, with the general behavior mentioned by plaintiff's expert in the cracking of shale oil, the results being just opposite from those obtained in the cracking of petroleum—the tendency being to become more saturated instead of less saturated as in petroleum products, and to get a higher viscosity-gravity relationship instead of a lower one.

All these indicate that both the nature of the stock and the reactions involved in the cracking are very much different from those of Burton's gas oil from petroleum. The boiling range of Riebeck's product is undoubtedly higher than that of Burton's, as well.

Q. You have pointed out these differences in the stock and also the fact that the Riebeck stock did give a red bottom, as shown by his publications. Even if it had given a black bottom would it have taught anybody that Burton's type of gas oil would give a black bottom when cracked to make commercial yields of gasoline?

A. No, they are two entirely different things. The only similarity really is in the fact that they are both called gas oil, one being used in Europe and one being used in this country.

Q. You were asked in cross-examination about the differences in cracking rate between oils of different molecular [fol. 1261] weights, and I am not sure the record has your full position, so I want you to state it briefly.

A. As I recall, our results indicate that for a given series of oils—that is to say, oils from mid-continent crude or oils from Pennsylvania crude, or oils distilled from pressure tar—that the cracking rate at a given temperature is approximately a maximum for the material boiling in the neighborhood of six hundred to seven hundred and dropping off slightly, although not very sharply, on either side.

But as I also mentioned, the cracking rate for all the members of one family may be higher or lower than that of the members of another family. That relation cannot be extended to cover one oil from one series or one crude and another oil from another series or another crude.

Q. Will you illustrate that by repeating the facts about the cracking rate of the Burton re-run stock which you gave in your direct?

A. A given cut of the Burton re-run stock, between two given boiling limits, cracks a little less than half as fast as a Virgin mid-continent oil of the same boiling range.

Q. You said something in your cross-examination about some trouble with California oils when they are run in Burton's stills. I want you to tell us about that trouble, what it is.

A. The California oils are characteristically high in sulphur compounds, and in the course of the cracking reaction that sulphur is liberated in various forms, to a considerable extent as hydrogen sulphide and that hydrogen sulphide attacks the still walls quite vigorously over a period of time. In other words, whereas we might lose an eighth of an inch in two years from our still walls due to corrosion, with mid-continent gas oil they would lose maybe a quarter of an inch in a year with California gas oil. As a result of this characteristic of California oils, the first attempts to use Burton shell stills on California gas oils, while they cracked satisfactorily and gave a black bottom

operation which was entirely satisfactory, the rate at which the stills were corroded away was so rapid that the necessity of replacing them in a few months, in order to preserve a satisfactory factor of safety, made the operation rather unprofitable.

I simply want to emphasize that while the California gas oil is a clean cracking stock, it does bring in another difficulty which Burton was not considering. In saying that the Burton process had to be dropped, I think the statement is not exactly accurate, because what was eventually [fol. 1262] installed was a Clark type of still which carried out essentially the invention of the Burton patent, but in the type of still which Clark shows in his patent, where there is a pump circulating the oil from and to the bulk supply. By using that expedient, and thicker walls on their stills, they have been able to crack large amounts of California gas oil with very good success. Of course, the corrosion problem is still not solved, and, as I indicated, they are working on chromium plating for the inside of some of their stills, in order to preserve the steel from attack by the hydrogen sulphide, chromium being quite resistant to that attack.

Q. And this corrosion difficulty, then, has nothing whatever to do with the hot bottom trouble that Burton cured?

A. No.

Q. And he cured it for California oil just the same as any other oil—the same for mid-continent—successfully?

A. Yes, the California gas oil may be slightly different, undoubtedly is slightly different, from mid-continent in its cleanliness, but it is perfectly possible to make commercial yields of gasoline in a commercial size still without a red hot bottom, and that is not the difficulty with the California gas oil.

Q. Plaintiff's counsel seemed in some doubt in his cross-examination about the actual size of the fired surfaces in the Humphreys still shown in the Humphreys patent No. 1,119,700. Just state what the fired surface is in a still of that type in this art.

A. These stills are all mounted substantially in accordance with the customary practice in the oil industry, which is to support the stills by means of lugs which are placed on a horizontal line approximately half-way up the still, as indicated, for example, in the Burton patent No. 1,049,667, and

the brickwork is brought against the side of the still below these lugs to make a tight seal, the top part of the still being covered by insulation of one kind or another. As a result, the fire plays on slightly less than half of the bottom of the cylindrical part of the still and does not touch the front end, and sometimes touches part and sometimes does not touch the back end. As a rule, in our cracking stills the back end is not exposed to any fire.

The Master: I do not know whether I understand you. These devices are riveted to the side of the still?

The Witness: Yes.

The Master: What you call lugs?

[fol. 1263] The Witness: Yes, and rest on the wall.

The Master: The wall supports those brackets?

The Witness: Yes.

The Master: And thus supports the weight of the still?

The Witness: Yes.

The Master: The walls being parallel with the length of the still?

The Witness: Yes. The walls are parallel with the length of the still. The tight seal is made just below those lugs, with the wall. So that the fired surface is somewhat less than half of the cylindrical portion of the still.

Q. You were asked some questions about the boiling point of fractions in the lubricating range, and stated that the true boiling point at atmospheric pressure could not be obtained in an ordinary distillation in a flask. I wish you would explain that situation.

A. If we try to distill anything heavier than gas oil in a laboratory flask, when we get above 725 or 750 degrees the rate of cracking becomes enough to affect the results to a considerable extent. That is, instead of measuring the boiling point of the original oil, we are measuring in part the boiling point of the decomposition products, and therefore it is hard to get an accurate figure. It depends on how fast we distill it, and by ordinary running at atmospheric pressure we cannot decide what the true boiling points are.

The way we arrive at those true boiling points is to make a distillation in vacuum so that we can get the temperature at which these oils distill off at certain very low pressures. Then we calculate what the approximate boiling point at atmospheric pressure would be by the use of the vapor pres-

sure curves which I presented earlier in my testimony. By that means we are able to speak of the fact that the lighter lubricants boil over the general range of 750 to 900 and the heavier lubricants somewhat higher, at atmospheric pressure, though it would not be possible to distill them at atmospheric pressure without cracking.

Q. In other words, you distill in a vacuum and then correct for the difference in pressure?

A. Yes.

Q. And in that way this theoretical or ideal atmospheric boiling point has been determined for most of these oils, has it not?

A. Yes. It is not accurate within two or three degrees, of course, but it is a good approximation.

[fol. 1264] Q. If you did not do it that way what you would really be doing would be giving the boiling point of some gasoline and kerosene which you were forming by cracking at the temperature?

A. Yes. The other method of expressing the same thing which is sometimes used is to give the boiling point at five millimeters or ten millimeters pressure, but the trouble with that is that everybody distills at different pressures and there is no standard pressure except that of one atmosphere, which is generally accepted and generally thought of. That also brings the oils into line with the other oils that we know, which we generally describe in terms of their atmospheric pressure boiling points.

Q. You were asked some questions I think yesterday about the acid treats used in making medicinal white oils, the proprietary product Nujol being particularly mentioned. I wish you would tell us what happens in the treatment of that kind of a product, how it compares with ordinary treats.

A. In the treatment of that kind of a product, in the first place, we use very strong acid, stronger than one hundred per cent,—that is, has some free SO^3 in it—and the acid of that strength by no means confines itself to attacking unsaturated hydrocarbons. For one thing, it has a very pronounced oxidizing action, actually oxidizing certain constituents in the oil, (we do not know just what), which it has been found desirable to remove. This is clearly indicated by the large amounts of sulphur dioxide given off in this treat, which shows that the sulphuric acid has been func-

tioning as an oxiding agent, being itself reduced to a lower oxide. That bears no relation to the ordinary unsaturation tests, in fact it is quite important that in making the ordinary unsaturation test you would not use acid stronger than the 93 per cent specified. We very frequently use 90 per cent acid just to avoid any danger of getting oxidizing or other actions taking place.

Q. I believe you said also, in connection with your cross-examination on this, although I have not had time to read it, that you took off fifty per cent or more; that is, you treated out fifty per cent or more of the original oil even though of good quality; was that right?

A. Yes.

Q. How does that compare with the ordinary treat or the ordinary sulphuric acid absorption on the same oil?

A. Why, it is very much higher than the sulphuric acid [fol. 1265] absorption. I do not know exactly what absorption would be given, but a negligible amount compared with the amount that we actually treat out with the strong acid.

Q. You also said you did not know what it was that was treated out; as I recall your testimony. Is that because of the chemical complexity of this situation, which you explained in the early part of your direct?

A. Yes. I might also say that we do not even today know just what it is we leave in.

Q. You do not just know what this white oil is that is sold?

A. No, we think it is largely naphthenic in structure, but we could not give any details beyond that. We know that we have destroyed many naphthenic compounds in the treating process.

Q. You were asked some questions yesterday about changes in viscosity of oils due to changes in temperature. I wish you would re-state, as I think you stated it generally in your cross, what is the general direction of the change in viscosity as you raised the temperature.

A. In general, and so far as I know invariably, you lower the viscosity of an oil by increasing its temperature.

Q. Does the amount that a given change in temperature changes the viscosity, remain constant with all kinds of oils?

A. No, it varies over quite a wide range. That is, certain oils are known to have comparatively small variations in

viscosity with temperature, and others have quite large variations in viscosity with temperature.

Q. Then, if you are given the viscosity of a given oil at one temperature and have not the oil before you to determine the rate of change, are you able from that one figure to determine its viscosity at any other temperature?

A. No, not, of course, unless you happen to know just what crude that oil is made from and happen to have figures on typical oils from that crude. Then you can approximate it with fair accuracy.

Q. You were also asked some questions on this same point about the temperatures for taking Engler viscosities. Have you gone into that question any further since your cross-examination yesterday?

A. Yes. Of course, the Engler instrument is an instrument developed in Germany and more generally used there. So I looked in Holde's book on the testing of mineral oils and fats. I find in the 1909 edition of that book, at the top [fol. 1266] of page 110, a statement regarding the use of the Engler viscosimeter that (translating) "Oils which are used at normal temperatures, such as spindle oil and machine oil are, as a rule, tested at 20 and at 50 degrees, and occasional tests are also made at thirty and forty degrees."

Q. That is centigrade in each case?

A. Yes, the temperature being centigrade temperatures. "Steam cylinder oils being tested at 100 degrees." That is in line with my statement that I knew the instrument was used at a variety of temperatures. I also know that in our own testing of road oils that we characteristically use it at temperatures of generally 25, 50 and 100 degrees.

And I also know that in a book by Engler himself he mentions making tests with the instrument at 150 degrees centigrade.

Mr. Rauber: May I see that Holde book?

The Witness: Yes. (Handing book to counsel.)

A. (continued). In fact, in a later book by Engler himself, there is a similar statement on page 102, which adds the fact that the cylinder oils are sometimes tested at 150 and 180 degrees.

That makes seven temperatures specified that the Germans use and the additional temperature of 25 centigrade which we use quite frequently in this country.

Q. Now, Mr. Wilson, you were cross-examined at considerable length with reference to the sulphuric acid absorption of these oils, crude oils, and in your answers you said something about asphalts in the oil, which I think was partly gone into in your direct. In the first place, I want to ask you, in the ordinary handling of oils how you determine the asphalt content?

A. The simplest and most accurate method is distilling with a large amount of steam or with very good vacuum until you have a bottom which is an asphalt of certain specification. Of course, it is variable, depending upon whether you want to make a light asphalt or a heavy asphalt, but we generally speak of the asphalt content as being the amount of rather low-melting asphalt which is obtained as a residue when you distill without substantial cracking.

Q. Then the asphalt content does not distill at all?

A. No.

Q. Does the asphalt react with sulphuric acid?

[fol. 1267] A. Yes, it does. If you use enough acid you can destroy it practically completely.

Q. The asphalts then are ordinarily gotten rid of by this distillation process, and I will ask you whether they are normally considered as the unsaturateds in the trade?

A. While it is generally known that they meet the unsaturated test, they are generally classified as a separate class of hydrocarbons, because they are so different from the other unsaturated hydrocarbons. In other words, in the petroleum industry we have two general classes of saturateds and unsaturateds and then aromatic hydrocarbons which are not considered to be a part of either class, and asphaltic hydrocarbons which, while probably unsaturated, are quite a distinct class which are handled in different ways.

Q. And the first three classes, as the trade uses the terms, saturateds, unsaturateds, and aromatics all distill off?

A. Yes, those are all distilled; products that distill over.

Q. And the asphalts always stay behind in the still?

A. Yes, except for entrained drops, or something of that kind.

Q. And so, if you take a distillate oil of any kind and test it with sulphuric acid, that test will show what the trade commonly calls the unsaturated content?

A. Yes, testing with ninety-three per cent acid, under specified conditions.

Q. Now, this examination, I assume, was directed to your comparison of this crude oil with shale and like oils of rock origin in Europe. What is the fact about these asphaltic fractions in the shale oil?

A. Shale oil sometimes contains tarry materials, particularly after standing around, but they are evidently quite different from our asphalts, because they have been produced by distillation. In other words, they are not the same type of materials, at all, because they have been distilled over—such as they have—of course they don't have as large a proportion as in most crude oils. If you simply take a crude oil and determine its sulphuric acid absorption you would get a superficially greater similarity between that and the shale oils, than is really justified, because most of the apparent unsaturation in any crude oil is really the asphalt, and that asphalt is not similar to anything in shale oil. The distinction which I drew was the fact that around ninety per cent of practically all American crude oils were either asphalt, or saturated hydrocarbons, neither of which [fol. 1268] were present to a very large extent in ordinary shale oil. Of course shale oil does contain——

The Master: In ordinary bulk still distillation, does the residue asphalt remain with the coke in the still?

The Witness: It ordinarily is cracked to make coke—excuse me—in the ordinary bulk distillation at atmospheric pressure—coking stills, you mean?

The Master: Yes.

The Witness: If it is carried to the coke stage, then the asphalt has all been converted to coke. The heavy lubricants and asphalt are cracked to make coke. If you use large amounts of steam, however, you can and will have asphalt with very little coke.

Q. To carry this comparison a little further, if you were to take shale oil and American crude oil with an asphalt content, and were to distill under a high vacuum, so as to avoid cracking, or with large quantities of steam, how would the residues compare in the two cases—in quantity?

A. It might depend on the vacuum. I would hesitate to say, not having actually distilled shale oil in high vacuum.

Q. What I am particularly getting at, is how would the distillates compare?

A. The distillates from the American petroleums would be characteristically very high in saturation. Whereas, this shale oil distillate would be characteristically highly unsaturated.

Q. These distillable ingredients of the two oils are quite different in their composition?

A. Of course all of the shale oil has been distilled from the shale.

Q. Now, you were asked a series of questions about an experiment of Dr. McKee's—which purported to show that the cracking of American oils, might take place at very low temperatures.

I wish you would state the actual basis and experience upon which you based your statement that American oils did not crack below a figure which you gave, and which, as I recall it, was between 625 and 650 degrees, somewhere.

A. I have done three types of experiments—well I have caried out four different lines of work which involved, incidental to that work, and in no relation to this case, the determination, pretty accurately, of where different types of petroleum oils begin to crack.

The first of these was in connection with the vacuum dis- [fol. 1269] tillation of lubricating oils, which has been carried on under my directon for several years, in trying to develop new methods and improved methods of handling and making lubricating oils. We found, in working in high vacuum, that the first traces of cracking are undesirable, because they form gas and tend to destroy the vacuum, or make it much harder to keep a good vacuum.

Furthermore, they tend to reduce the viscosity of the lubricants, and in that work we have almost invariably found that we are likely to get into trouble with gas formation, if we hold the body of the oil at 650 degrees, but that below 625 we are quite safe.

We have also found, in the same connection, that working in a tube still, in vacuum, we can go to around seven hundred degrees, before we get appreciable amounts of gas given off from any of a wide variety of types of oil, because there the time element is much smaller than in distilling from a bulk supply.

That has also been extended to work in commercial pipe stills where we are anxious to avoid cracking. And there we find that about 725 to 750 degrees is the limit we can go to without appreciably hurting the viscosity of our product.

Probably we have got a little cracking at that temperature, or a little gas formation, but in working at atmospheric pressure it doesn't bother us very much.

Q. In that the time limit is extremely short?

A. In that the time limit is extremely short, of course.

Q. How long, aproximately?

A. Oh, a matter of a few seconds, so far as being within the range above of seven hundred degrees is concerned.

In addition to that, in connection with work on the Dubbs' case, we operated the apparatus many times, at temperatures up to around 500 degrees, or a little higher, and carefully tested the products for any indications of cracking, in connection with the dehydrating process, and found no indications of cracking under this condition.

There was sometimes a change in the odor of the product, probably due to very slight decomposition of sulphur compounds, but no cracking of the hydrocarbons, so far as we could detect.

In addition to that I took different hydrocarbons and put them in bombs and immersed them in a lead bath, and studied the rate of cracking at different temperatures, finding out the differences between the different types of hydrocarbons.

[fol. 1270] I have already pointed out the contrast between the comparatively stable aromatic hydrocarbons and the highly unstable hydrocarbons present in turpentine, which lie on either side of the petroleum hydrocarbons.

In addition to this I have discussed my conclusions with men who have done other types of work, like making asphalt, and have had experience with other types of oil, and find their results were substantially the same as mine.

Q. You were asked some questions about the method by which you determined the amount of vapor loss which was saved in the Burton process by pressure condensation, as compared with what would have occurred had you condensed at atmospheric pressure.

I wish you would explain that in a little more detail, please.

A. The first necessary step, of course was to determine how much gas we separated, under pressure, in a normal operation, and we found out that this amounted to approximately 4.3 cubic feet per gallon of gasoline made. We then collected a typical sample of distillate in a drum, at 95 pounds pressure, in contact with the gas, this drum being equipped with a gauge glass, and it being calibrated to measure the exact volume.

By means of pressure from the high line we then forced a measured quantity of the liquid from this drum into another drum at atmospheric pressure, there losing a certain amount in volume.

In other words, we didn't get as much volume in the second drum as we had in the first, that loss being due to the separation of the gas which was dissolved in the oil at 95 pounds pressure, before it came out at atmospheric pressure.

Then, having measured that volume for the loss due to the gas which was dissolved,—which, of course in our process is recovered by our scrubbing plant—we passed the much larger quantity of gas, namely, the 4.3 cubic feet per gallon of gasoline, bubbled it through this other receiver at atmospheric pressure, thus finding out how much more light ends would have been removed by that cause, had that additional larger amount of gas been separated at atmospheric pressure, and we found that the loss that that produced was 8 per cent of the gasoline present.

Q. You were asked some questions in cross-examination about the products of the gas-making process, and stated that that process made some gas and some liquid; also, that [fol. 1271]. the Burton process, for example, made some gas and some liquid. I want you to compare the quantities of these gaseous and liquid products in the gas-making processes and in the gasoline cracking processes of the defendants, particularly the Burton process.

A. The gas-making processes characteristically make only 15 to 20 per cent of total liquid products, some of which are quite tarry in nature, and practically all the rest is gas.

The Master: Because you are trying to get gas.

The Witness: Because you are trying to get gas, and you are cracking under radically different conditions.

The Master: Yes.

The Witness: In the Burton process a typical run will produce around two or two and a half per cent of gas and a tenth of a per cent of coke, and all the rest of it is liquid products, either distillate or pressure tar in the still, which can be treated to recover asphalt products. The ratio between the two yields of gas is in the order of thirty to one.

Q. You were asked some questions about the use of dirty stocks in the Holmes-Manley process, and also about the use of dirty stocks in the tube and tank process. I wish you would explain that a little more fully.

A. I made clear, I believe, that the tube and tank process, while largely limited to clean stocks such as those of Burton or Chamberlain, was sometimes used for doing very light cracking on heavy oils, mainly for the purpose of lowering the viscosity of those oils, making only a very few per cent of gasoline. I neglected to say that, of course, the same operations can be carried out in the Cross and in the Holmes-Manley type of process, where most of the heating is done in the tubes and most of the cracking or substantially all of the cracking is done in the unfired or lightly fired reaction vessels. If you are willing to sacrifice gasoline yield you can handle, in these processes, considerably dirtier stocks, including reduced crudes, than you can possibly handle in the Burton process, even for similar light cracking; but if you want to make the yields of gasoline such as are customarily made from clean stocks you cannot handle these dirtier stocks and get runs of more than a few hours or a day.

Q. You were asked yesterday afternoon some questions about the critical temperatures of some hydrocarbons. I wish first you would tell me what critical temperature means.

A. The critical temperature of a liquid is the tempera- [fol. 1272] ture above which it cannot be liquefied by itself; that is, no matter how much pressure is put on, if the temperature is above a certain critical temperature, it cannot be held in liquid form by itself. Of course, that does not mean that it cannot be dissolved in some other liquid constituent which may be present. In fact, some of

our common gases, are above their critical temperature, yet they will, of course, dissolve in other liquids and really be in the liquid phase at temperatures above their critical temperature.

Q. And that is true, for instance, of such oxygen and carbon dioxide as is dissolved in water, at 80 degrees Fahrenheit, isn't it?

A. Well, they are not above their critical temperatures. Carbon dioxide, for example, is not above it——

Q. At 80?

A. Oh, at 80 degrees Fahrenheit? Yes.

Q. Or seventy-something?

A. I have forgotten exactly what the critical temperature of carbon dioxide is.

Q. Well, at a hundred it would be?

A. I think so.

Q. What data is available on the critical points or temperatures of hydrocarbons?

A. I think the actual determinations of the critical temperatures of hydrocarbons are quite few in number. In my paper on the physical properties, which has been offered in evidence, I showed the critical points which had been determined, and then calculated by thermo-dynamic means, the best approximation I could make of critical points for the higher paraffin hydrocarbons, though these have never been actually measured. They probably are fairly accurate for the paraffin hydrocarbons, but as for applying them to the complex mixtures which we have in petroleum, I do not know of any commercial mixture where we know enough about the hydrocarbons which are present and the relative proportions of the different hydrocarbons and the critical data of the different hydrocarbons to make it possible to calculate within a hundred degrees or more, if that closely, what the critical temperature of a given mixture would be.

Furthermore, even though the lighter paraffin hydrocarbons present in gasoline might be, under certain conditions, above their critical temperatures and possibly even the other hydrocarbons in the feed stock might be above their critical [fol. 1273] temperatures, we have formed in the cracking reaction a large amount of heavy constituents which are unquestionaly well below their critical temperatures, and those heavy constituents will at high pressures dissolve large quantities of the lighter hydrocarbons, even though the latter may themselves be above their critical temperature.

So that we cannot predict from any critical data that I know, even if we had accurate composition data, just exactly what proportion will be in the liquid form and what proportion will be in the vapor form at temperatures around 800 or 900 degrees Fahrenheit.

Q. Empirically, what is the fact about the presence in liquid form at these temperatures of gasoline in the cracking process?

A. In such cases as I have taken samples of liquids at 750 or 800 degrees, of the tar in the pressure still, I have found materials dissolved in the liquid form, which are very likely above their critical temperatures, at least they would be above their critical temperatures if they were paraffin hydrocarbons, indicating the solubility of these hydrocarbons in the oil, which is liquid at those temperatures.

Q. Do you find that under these pressures and temperatures gasoline is actually in the liquid form in the tar?

A. Under many conditions, yes; if there is a large proportion of gasoline in the apparatus there is always a considerable amount in the tar.

Q. And if there is a large proportion in the tar there is necessarily a considerable portion in the vapors?

A. Yes.

Q. You were asked some question about the marketability or oils that were marketed; does the fact that an oil is marketed indicate that it is free from the gum-forming constituents described in the Ellis product patent?

A. No. As I pointed out, even today when we are watching those things more closely, there is a considerable amount of material marketed for motor fuel which is not free from gum-forming constituents, although the product put out by the larger companies is substantially free.

Q. That is, a gasoline may contain considerable quantities of gum-forming constituents and be marketable but it would be better if that was taken out, is that right?

A. Yes.

[fol. 1274] Q. Do you find in the prior art any disclosure of any treat which does take out gum and leave the olefins?

A. Disclosed as so doing, you mean?

Q. Yes, which discloses doing that?

A. No, I do not.

Q. You were asked some questions about the Atwood patent and the Burton safety valve. At what pressure

would you, as one skilled in the art, understand that Atwood runs his still?

A. He runs his still at atmospheric pressure and probably the safety valve is set for a pound or two pounds above atmospheric pressure.

Q. The Burton still runs at 75 pounds or thereabouts, or more?

A. Yes.

Q. Four or five atmospheres or more?

A. Yes.

Q. Now, if the safety valve blows, do both of them give condensable vapors and some gas?

A. Yes.

Q. What is it that Atwood wants to do?

A. Atwood is trying to recover as much as possible of his condensable vapors by passing them through a large cooling chamber, in which he hopes to collect and draw off the condensable portions, allowing only the gas to escape through the outlet pipe.

Q. Is that possible and feasible at one pound pressure?

A. Yes, entirely so.

Q. Burton, on the other hand, wants to do what?

A. Burton wants to liberate an enormously larger quantity of vapors, and in practice while he might theoretically like to condense, would not be able to condense it all because the size of the condenser would be out of all question, and if he attempted to do like Atwood did, that is, partially condense in an enlarged chamber, he would be worse off rather than better off, because some of this partially condensed material would be swept along by the tremendous flow of gas and sprayed out as liquid drops over the surroundings; the whole essence of Burton's invention is in cooling just enough to eliminate the danger of auto-ignition and yet not condense any liquid drops.

Burton could not do what Atwood did. Atwood's device [fol. 1275] would not be suitable at all for Burton's operation, because of this fact.

The Master: In other words, Burton is trying to waste his vapors and reduce his pressure as quickly as possible and convey the vapors away beyond the fire range.

The Witness: In the vapor form and not partly liquid.

The Master: Yes.

The Witness: Yes.

Q. You are asked in your cross-examination about whether the Clark tube still invention was present in the Holmes-Manley process, and I will ask you what operation of the Holmes-Manley you were considering when you answered that that was not present?

A. I was considering the normal operation of the Holmes-Manley process during the running period and did not, for the moment, realize that in the starting up period they do the characteristic Clark operation of rapidly circulating from and to a bulk supply of oil, even for some time after this bulk supply of oil is at a cracking temperature.

Q. They pump round and round until the tar begins to get a little dirty?

A. Yes, they pump round and round until they have done a considerable amount of cracking, and then they start the normal continuous operation which I had in mind, but in this early stage, which lasts a good portion of the first day, they are operating essentially in accordance with the disclosure of the Clark tube still patent.

Q. I want you to state, if you will, what one skilled in the art would, in your opinion, understand to be the purpose and function of the drum at the upper part of the Schuchow and Gavrilow apparatus?

A. One skilled in the art would find it very difficult to determine clearly from the description just what the entire purpose and what the operating conditions in this drum were.

To illustrate the indefiniteness, I point out that plaintiff's expert cited this operation as disclosing both the Ellis tube and tank of cracking, which would require that the tank A be kept at a rapid conversion temperature, and against the Humphreys' runback patent, which would require a very substantial amount of cooling, seven hundred degrees below a cracking temperature, to condense and return the vapors heavier than gasoline, assuming he were making gasoline.

Furthermore, the disclosure is one either of fractional distillation without substantial cracking or for what he calls "the recovery of gas" from petroleum and its products. Apparently, when he is considering cracking, he is considering the ordinary type of tube cracking operation for the making of gas.

I might also point out that a good share of his disclosure is taken up with the operation of the fractional condensers [fol. 1276] for separating the ordinary products of distillation, these being shown at the right of the figure on page 1621.

With regard to the conditions in the drum, there is no statement as to what temperature it should be kept at, nor how much feed oil should be put through it, or anything which would enable you to arrive at any particular method of operation.

I presume, if he wanted to make gas, he would naturally try to condense everything but the gas in that chamber and recirculate it around, in which case he would of course do very different from Humphreys; but even if we should read into the disclosure an attempt to take off gasoline and make gasoline and return the other materials to be further cracked, we have simply and solely the return of the heavier parts of the distillate to a tube cracking process, which is old in a number of examples already cited in the art, and has no relation to the very difficult problem which Humphreys solved, of returning a large amount of condensed vapors to the body of oil in a bulk pressure distillation process.

I pointed out that the exact composition and efficiency of stripping of the product returned in the Humphreys case is extremely important in its effect on the nature of the process, and that simply returning the distillates to a tube cracking process involves no particular problem, because we have no need for a correlation of temperature and pressure. When you come to return that to a bulk supply of oil it is a very difficult problem, and it is only by doing it in a particular way, involving careful fractionation of the distillate, as Humphreys discloses, to get the desired produce, and then very thorough stripping of the light ends that you can accomplish the desired purpose of returning to the still all the insufficiently cracked material and yet maintain a high enough temperature in the body of oil to bring about a rapid cracking rate.

Q. The reference against the Ellis patent is that it has got to be very hot and the reference against Humphreys is that it has got to be cool?

A. Yes; and, even then, it would not be a reference against Humphreys for the reasons that I mentioned.

Q. The plaintiff's expert cited it against both.

A. Yes. As to the Clark tube still, of course, we lack the function of holding a large body of oil at a cracking temperature outside the heating zone and allowing the coke to [fol. 1277] deposit there. There is no such showing whatever here. The outlet pipe is right near the bottom, and there is no provision in a continuous process like this for holding a body of oil in A. It is apparently a dephlegmating and separating chamber, mainly to stop entrainment, and partly to heat up the feed by the waste heat of the vapors coming up into that chamber.

Q. Comparing this with Humphreys; in this process both the tar and the vapor go up into this separator, do they not?

A. Yes.

Q. And in Humphreys only the vapor?

A. Yes.

Q. And in this process the principal thing returned would necessarily be tar, would it not, or a very large part of it?

A. A very large part of it. All the dirty material would be returned.

Q. Quite different from Humphreys' returned material?

A. Yes. It would also include the dirty feed stock.

The Master: Let us see what Humphreys you are referring to. I may not be thinking of the same thing.

Mr. Wiles: The runback.

The Witness: The Humphries runback.

Mr. Wiles: Patent No. 1,119,700.

The Master: That is what I thought you were referring to. Let me understand something about it. This pipe D leading out of the Chamber A and going down to the pump and back through the heating coils again, isn't that more analogous to refeeding the cycle stock to the still or tubes?

The Witness: No, because there has been no cleaning-up action taken place. I mean, if you are referring to an analogy to Burton's operation, there has been no cleaning up of the cycle stock to make a clean stock by depositing the coke outside the still, as I pointed out was the essential feature of the Burton re-run patent. It is simply the characteristic recirculation through the tube which is so generally used in tube-cracking processes. Sometimes they re-

circulate the tar, sometimes the heavy distillate, and sometimes everything, like in this case—just keep re-circulating around and around through your tube, and then have a separating chamber on top.

Q. In the Clark feeding-the-runback patent the material there treated is all vapors—I mean that enters the runback from the still from the heating zone, is all vapors, is it not?

A. Yes, and it is a clean stock that is put into the runback, of course.

[fol. 1278] Q. Yes. So the thing that comes back is a mixture of clean feed plus condensed vapors?

A. In Clark, yes.

The Master: The symbols in this Russian patent are a little bit confusing to me, and I would like to know what products are supposed to be separated out by the condensers on the right of the drawing.

The Witness: The ordinary products of fractional distillation are what he is concerned with there—that is, benzine, and kerosene—probably making different cuts.

The Master: Where does he draw off his different cuts? There is only one final receptacle there. The final receptacle is N, isn't it?

The Witness: No, because he has little reservoirs, you see, in the bottom of each of those partial condensers.

The Master: But wait: What are these wiggly lines that are running up through each one of these compartments, F, F and F?

The Witness: That is the line through which the water spra*k* goes in. You get some warning of the water by indirect contact, and then you get the direct water spray.

Q. Thus warming up the water and cooling the vapors?

A. And cooling the vapors. Then you shoot the spray directly into the chambers. I understand the products would be withdrawn separately from those chambers.

The Master: He says that he makes six separations here, one for each one of these chambers.

The Witness: He says on page 1623:

"The separators for the distilled products consist of an arbitrary number, according to the number of products which are being recovered."

and goes on to describe these cylinders. That is near the bottom of page 1623. So I assume he would vary that, depending on the number of products he wished to distill out.

The Master: Isn't that on page 1624?

The Witness: Well, page 1623 I was reading from.

The Master: I see it—at the bottom.

The Witness: Yes, the sentence beginning, "The separators for the distilled products."

Q. That is a diagrammatic illustration of a normal form of fractional condenser, is it not?

A. Yes.

Q. I mean in principle it is a known method of separating in a pipe still type of apparatus, is it not?

[fol. 1279] A. Yes. He emphasizes particularly being able to control just exactly what he is going to get in each of those chambers by controlling his cooling medium.

Q. In the ordinary pipe still, instead of taking off one cut after another, as your temperature rises, you take off everything at once and then condense cut by cut; is that correct?

A. Yes, whereas in the ordinary batch still you take off the lighter cuts first and the heavier ones later. In a pipe still we have a whole mass of vapors, heavy and light, and then we condense out the heaviest, then the next heavier, and then the lightest.

Q. Making your separation by progressive cooling of vapors instead of progressive heating of liquid?

A. Yes.

Q. And this is a typical device of that kind?

A. Yes, except that as a rule the pipe stills do not have pressure on their separating drums.

Q. He also discloses using this for gas-making does he not?

A. Yes, he also discloses the use of this for making gas.

The Master: Where does he hold his pressure?

The Witness: He holds his pressure with the valve M.

The Master: Where is M?

The Witness: At the top. The pipe going out the top.

The Master: I thought he held his pressure in A.

The Witness: M is what primarily holds the pressure on the system.

The Master: How does it control any pressure beyond the valve?

The Witness: Oh, there is no pressure on the condensing part of the system.

Q. Continuing the comparison of this with the Clark patent on feeding the runback, you have stated that Clark returns clean feed, puts into the still at the bottom of the runback clean feed and condensed vapors, which would also be clean. What does this put back?

A. This puts back a mixture of a dirty feed and some still dirtier tar, and some condensed vapors—quite a different composition of material, of course.

Q. I am assuming that this patent may also be used for its bearing on the Burton and Clark patent No. 1,448,254. You stated in your direct that the function of this invention was to put the reflux directly back into the tubes, thereby decreasing the quantity of partially dirty stock which had [fol. 1280] to go through the tubes. Is such a function present in Schuchow and Gavrilow?

A. No, in this case your feed stock is itself dirty and you are putting back with it the same amount of tax as if you were not putting any feed or reflex in at all. All the tar goes right back through the tubes regardless of whether or not you are putting the feed through.

Q. So that whereas Burton and Clark decreased the amount of tar flowing through the tubes, this puts all the tar through the tubes?

A. Yes.

Q. This was also used on Ellis, apparently, with the assumption that the chamber at the top was hot. You pointed out, I believe, in your direct that Ellis had three parts. What were those?

A. First, the heating and cracking tube; second, the large unfired drum held at an active cracking temperature but lower than that of the outlet of the coil; and, third, the separating chamber to separate the liquids from the vapors.

Q. Beyond that there was the final condenser, of course, for the vapors?

A. Yes.

Q. And you pointed out that Ellis' novelty consisted largely in the fact that he put between the tube and the separating chamber another element?

A. And that he controlled the temperature of the oil in that element very closely to yield a highly desirable result, of such additional cracking away from active heating.

Q. Has this Schuchow and Gavrilow process that added intermediate part at all?

A. No, it has not.

Q. What has it?

A. It has simply a large separating chamber with no statement as to what temperature is maintained therein, and with a draw-off near the bottom, which would indicate that no substantial body of oil was held in the chamber. The entire implication, of course, from the fact that it is cited against these other patents, is that the temperature in there is quite low.

The Master: Which one of these Ellis patents were you talking about?

The Witness: The tube and tank patent.

The Master: What is it, and what is the number?

Mr. Wiles: I think it is No. 1,415,232.

[fol. 1281] Mr. Rauber: Exhibit No. 192.

The Witness: You will recall that the separating chamber is in the base of the tower. He has the coil, and a chamber held at an active cracking temperature, but only very lightly fired, inserted between the coil and the customary separating chamber of the prior art.

Q. Mr. Wilson, in your examination you were asked to compare Cross and Ellis—I think the Master asked that question—and I want you to go over that in a little more detail than you did before.

A. At that point, in speaking of the substantial identity of the Cross and the Ellis operation, I was of course referring to the features disclosed by Ellis. That is, Cross does clearly have the feature disclosed by Ellis of first heating his oil quite hot, to a cracking temperature, in a tube, and then passing it into a large chamber inserted between the tube and the separating chamber, in which reaction chamber

substantial conversion takes place without the application of heat and with a lowering rather than a rise of temperature.

The Master: In Ellis?

The Witness: In Ellis, with very light application of heat.

The Master: I know, but there is an accumulation of heat in Ellis' reaction chamber, is there not?

The Witness: Ellis emphasizes the fact that his reaction chamber temperature must be lower than the tube outlet temperature, and in no case higher. In other words, you must have only very light heating in this chamber where the bulk of the coke is deposited.

The Master: He means the heat applied to the chamber is a light heat, but as a matter of fact in Ellis' reaction chamber, where his material goes through the coils and is heated, there is a rise of temperature of that material in that reaction chamber; isn't that so?

The Witness: No, no. Ellis makes it very clear that the temperature drops in the reaction chamber. The gases [fol. 1282] which are around it have been cooled by passing the tubes, bringing in the cold incoming oil, this oil being far below a cracking temperature; so there is only a very small amount of heat put in the drum. And Ellis makes clear that the temperature in that drum should be somewhat lower and in no case higher than the temperature at the outlet of the coil. That is the essential feature, and that is a measure of how hard he means to fire, if at all.

The Master: Then, equally, wouldn't there be a drop in temperature, of the material in the reaction chamber of Cross?

The Witness: Yes, there would be a slightly greater drop in temperature there. That is the way in which they are substantially similar. I, of course, did not mean to indicate that Cross does not have additional and important features beyond that of Ellis. I have particularly in mind the two features that I have pointed out—first, that Cross, by using a very rapid flow postpones practically all the cracking to the reaction chamber, and does substantially no cracking in the tube, which is an improvement over Ellis, and, secondly, that Cross uses very high pressures, to keep the greater part of his products in a liquid state, even in the reaction chamber. Those are two important improvements which Cross

has made above Ellis. In addition to that, there are certain other features.

The Master: What is that Clark—tube still—what patent is that?

The Witness: That is No. 1119496. Were you thinking of the Burton and Clark patent?

The Master: Yes.

The Witness: That is the one that has the boot in addition, and that shows the form that the Clark still actually took—that is No. 1148254.

The Master: Now, what is the distinction you draw between this, No. 1148254, and Cross?

The Witness: Well, Cross doesn't circulate from and to a bulk supply. You see, the defect in this older type of operation, while it was much better than the shell still, was that you were circulating dirty stock through the tubes, the stock being progressively dirtier and dirtier, and being circulated around and around from and to the bulk supply, contaminating the feed, and getting dirty stock on the fired surfaces of the tubes, which is what we want to avoid.

[fol. 1283] Burton & Clark took the first step over Clark, toward eliminating that, and by putting the reflux into the tubes, decreasing the amount of tar which was circulated.

Mr. Wiles: And Ellis went the whole way, and put straight clean stock through.

Mr. Rauber: Did the witness answer that last question?

Mr. Wiles: Oh, no, but it might be shown as withdrawn.

The Master: If you took that reflux out of Burton and Clark you would have Cross, wouldn't you?

The Witness: No. Burton and Clark is circulating around and around.

The Master: I know, but if you took that circulation out of there you would have Cross, wouldn't you?

The Witness: You still have got the reflux, which is another difference. There are several differences, you see. Of course, Burton is a bulk pressure distillation process, which has characteristic differences from the tube and tank type of process.

Q. You were asked, in your cross examination, for some definitions of mineral spirit. Have you verified your definitions?

A. Yes. I indicated that the only use I knew of the term was for paint thinners—varnish makers' and painters' naphtha, as we more ordinarily call it.

I have before me Circular No. 98, of the Bureau of Standards, which gives recommended specifications for volatile mineral spirits, for thinning paint, and I note that the amount of distillate below 266 degrees Fahrenheit, shall not exceed 5 per cent.—indicating that it is very far from being light enough to be satisfactory as a motor fuel.

The Master: You referred to that publication before, didn't you?

The Witness: No.

The Master: You referred to one publication defining mineral spirits.

The Witness: No, I was not able to say definitely that that was the only product the term was applied to. We don't use that term.

Q. You call it V. M. and P. do you not?

A. We call it V. M. and P. naphtha, as a rule.

Q. I wish to ask you one or two questions on the Barbet operation.

I believe you stated, near the end of your cross-examina- [fol. 1284] tion, that Barbet took off no gas oil cuts. Please state clearly just what does become of the material boiling between 600 and 700 degrees Fahrenheit, which Barbet produces, and what becomes of any material in this range, which is formed in the presusre still?

A. The material boiling in the gas oil range, which is 600 to 700 degrees Fahrenheit, which Barbet makes, as I pointed out, goes to either one of two of the cuts which Barbet does take off. Probably the major portion of the gas oil constituents present are taken off in the heavy kerosene cut, the low grade product which Barbet takes out through the box, 8, here. The remainder of the gas oil cut very clearly goes with the paraffin distillate, in order to make it easier to press.

In other words, Barbet was simply doing what the American refiners characteristically did when making the maximum yields of kerosene, which was to put quite a bit of gas oil into the kerosene, and also a large amount of gas oil into the paraffin distillate.

For example, even today, our paraffin distillate is actually more than half material which is substantially gas oil, and has no lubricating value, but which is left in, in order to make it possible to press the paraffin distillate, and is later removed and gets back into the gas oil. It is then put into the secondary gas oil cut.

Undoubtedly, when Barbet takes off this paraffin distillate and presses it, and then distills it to recover his gas oil, he does recover a large percentage of gas oil. It is also undoubtedly true that the heavier kerosenes contain a considerable amount of heavy oils, which we would ordinarily use as gas oil, even though, today, we do make illuminants from substantially the gas oil cuts.

As to the products boiling in this range which leave the pressure still, I want to point out that Barbet puts it in one of two places, and he has no cut between heavy kerosene and paraffin distillates, because he is worrying about separating the illuminating products from the paraffin products, and any material in the gas oil range, which is formed in the pressure still, will pass up through here (indicating) and condense out everything heavier than the illuminating vapors, in the condenser, R.

These materials which he refers to as the paraffin products, pass out through the cooler, V.

The illuminating vapors are the only things which are [fol. 1285] disclosed as passing over from R, everything heavier being taken out through V, or through *x*.

These illuminating vapors pasing over into the tower, are classified into three kinds of kerosene—the light, the intermediate and the heavy.

If we should depart from Barbet's disclosure, or if, due to imperfect operation, there should be a little bit of material heavier than kerosene, passing over into this tower, we find, in the first place, that Barbet says that all illuminants, and even a little of the paraffin products, are vaporized on the plates, H, it presumably going up in receiver O", and the heavier products, freed from their illuminants, pass down into the still M, and there, again the lighter portion of what comes in is distilled out. Thus, if some heavier material came over with the illuminating vapors, and was not vaporized in H, it would be the lighter vapors in M, and pass out through the condenser G and the cooler K, which forms the light paraffin distillate cut.

That means that except for traces, due to imperfect separation, the product passing over through *t—t'* are only products which are heavier than the light paraffin distillates, taken off through K. I presume that it might be argued that a few molecules, relatively speaking, of the material which came out of the pressure still, due to imperfect separation, due to not following Barbet's disclosures, might, by chance, not be vaporized in M and pass over into the pressure still; but if they did that is so utterly different from any of the operations which it is cited against as to be entirely immaterial; because, as I pointed out before, the Burton re-run patent, which this is mainly considered against, is a clean stock patent, and as I have also pointed out, the mere presence of a trace, or even of a substantial quantity, of clean stock as part of a very dirty stock, does not in any sense anticipate the Burton operation.

The Master: He asked you how you were going to start that on your direct examination, and you suggested that you would charge the cylinders M at the beginning of the operation.

The Witness: Yes.

The Master: Probably with crude stock, is that right?

The Witness: Yes.

The Master: Well, if you did that, started your fire, you would have a considerable operation carried on there in M, [fol. 1286] wholly independently of the operation, carried out through the lefthand part of the apparatus, beginning with the cylinder P, would you not?

The Witness: Yes, that would be the way I would assume you would start it up.

The Master: Yes. Now, then, if you did not start it that way but depended upon the condensed liquids running down into M, and that would have to proceed until M was partially filled before putting on your fire, then during all that operation you would have no tower at all with any heat in it on the lefthand side of your operation.

The Witness: That is correct. I assume you would probably have all your vital products in here (indicating) before you sent them over into the tower P. Probably after you operated and started the feed into the righthand part of your system for some time and got a heavy tar, such as

you wanted, to put into your pressure still, then you would start pumping over into here (indicating), but you would not fire the pressure still until it was nearly filled with feed, which had accumulated. Then the lefthand tower would begin to be warm and you, for the first time, would have the full continuous process which Barbet is mai-ly devoted to disclosing.

The Master: Then the pressure tank P operates only on the heavy tar, which is the result of the operations carried on in M?

The Witness: Yes; heavy tar from which all the illuminants, all the material in the gas oil range, except as I say, possibly a few molecules and much of the light paraffin distillate, has previously been removed. What goes through there (indicating) is substantially heavy paraffin distillate tar and asphaltic and other heavy materials.

The Master: Even all of the illuminating vapors that are carried over here from R, are such illuminating vapors as are left in the tar taken out of M?

The Witness: No, they are not left in there. We undoubtedly took out all the natural illuminating vapors there and what you take out through the line R, are the products of cracking. That is, in this still you have made some kerosene by cracking, and it is only those cracked illuminating vapors which are probably smaller in proportion than the vapors which distill off regularly are passed through the line *t—t'*; the other illuminating vapors are vaporized on the plates of H as the crude originally comes down, or vaporized in the re-boiler G.

[fol. 1287] The Master: Then M is merely an atmospheric distillation before it goes over in P, and you would have pressure distillate over in the lefthand side.

The Witness: Yes.

Q. While we are on this point, Mr. Wilson, I want to be sure that the record is absolutely clear on this primary and secondary gas oil cut as normally made. The primary gas oil, as I understand it, is that which is made by the crude still?

A. Yes.

Q. And it is the lighter half of your final composite gas oil, lighter part of it?

A. Well, not entirely the lighter part. It is what you

do not need or do not want either in the kerosene cut or in the paraffin distillate cut.

Q. Yes. Later, after you have made your primary gas oil cut you take—and it is only that one that you said sometimes disappeared entirely?

A. Yes, it is only the primary cut that sometimes disappears entirely. We always have a secondary gas oil cut after the gas oil has served its useful function with the paraffin distillate.

Q. Now, the secondary gas oil is the lighter parts of the paraffin distillate cut, is it not?

A. That is added to the primary cut to make the secondary cut.

Q. I will use refinery terminology. You take your paraffin distillate out and chill it?

A. Yes

Q. And then press it?

A. Yes.

Q. And then you steam reduce your pressed drips, is that correct?

Q. Pressed drips being the filtered, chilled oil?

A. Yes.

Q. Now, the light part of the distillate from the pressed drips goes back into gas oil?

A. Yes, this amounting to generally more than half of the pressing distillate itself.

Q. Yes. So that the gas oil as a whole is a mixture of whatever you took out of the crude stills intermediate between kerosene and paraffin distillate?

A. Yes.

[fol. 1288] And the light fractions of paraffin distillate after you have got the wax out?

A. And the heavy fractions from re-running the kerosene.

Q. And the heavy fractions from re-running the kerosene. Those three make up the complete gas oil?

A. Secondary gas oil cut.

Q. Yes. And that is the final product that is put into the Burton stills, is it not?

A. Yes.

Q. That is the residue that is left over after you have taken your maximum of gasoline, kerosene, lubricants and wax?

A. Yes.

Q. Now, this primary cut taken at the crude stills is the only part of it that disappears at any time when you are running for the maximum of kerosene?

A. Yes. In that case you frequently do not get a primary gas oil cut.

Q. At the crude stills?

A. At the crude stills.

Q. But you did not intend to indicate that there ever was a time when no gas oil was made?

A. No.

Q. These other sources of gas oil would always have been present?

A. Yes, that is true.

Q. Now, Barbet would inevitably produce, but not in this apparatus, this other gas oil from his paraffin distillate, would he not?

A. Yes.

Q. And all you are insisting on is that he has no primary gas oil?

A. Yes. He clearly is not operating to make any cut intermediate between his heavy kerosene, which undoubtedly contains material boiling from 600 to 700, and his paraffin distillate which also characteristically contains material boiling from 600 to 700.

Q. Your attention was called in your cross-examination on the Barbet patent to the fact that water of 70 degrees Fahrenheit——

A. Centigrade.

Q. Oh, yes, 70 degrees centigrade. Translate that for me quick.

A. That is——

[fol. 1289] Mr. Rauber: 158.

Q. 158 Fahrenheit was used in the cooler V to prevent the separation. What does that prove, if anything?

A. Well, if that statement is correct it simply indicates that Barbet must have been working, as we rather suspected, with shale oil petroleum. That is, his petroleum was shale oil, because I am quite sure there is no paraffin distillate cut from petroleum that does solidify at temperatures as high as 158. It is, of course, also possible that Barbet's statement was not strictly accurate; that he

was specifying a water temperature higher than really required to prevent chilling out any wax. It is true that our ordinary paraffin distillate cut, practically regardless of the amount of gas oil we have, will solidify if you use ordinary cooling water.

Q. That is about what, 50, 60 or 70 degrees Fahrenheit?

A. In that vicinity, depending upon the weather conditions and location.

And you must be sure to have the water warmer than ordinary cooling water to prevent solidification, but if it is really true you had to have water as hot as 158 Fahrenheit, that indicates that it was not a product containing the type of wax which we get from our petroleum.

Q. What bearing on this opinion of Barbet, as dealing with an entirely different kind of oil, would the fact have that Barbet says that he can press the tar that comes out of the bottom of the column Q?

A. Well, as I pointed out, that indicates, first, that the cracking must have been very light, and even with very light cracking I do not think that would be possible with any crude oil with which I am familiar.

Q. You were cross-examined about the possible effect of the Barbet towers in effecting the polymerization of gum-forming constituents in gasoline. I want you to state fully the facts about that?

A. I do not see how we can be sure whether or not there may have been a slight polymerizing action on the gasoline. The conditions are of course radically different from those in the Lewis and Cook bubble tower, where there is a very marked and very valuable polymerization taking place.

In the first place, the Barbet tower is at atmospheric pressure, and hence, at a much lower temperature, and as I pointed out, the polymerization which takes place under those conditions is much less.

[fol. 1290] In the second place, the amount of gasoline made is quite small, especially since only the lighter products go into gasoline. What we would today consider the heavy end of gasoline undoubtedly went into Barbet's light kerosene cut; therefore, the benzine or gasoline cut which would be obtained would probably contain so little gum that it would not make any particular trouble in any case.

Furthermore, the gasoline is mixed—the little gasoline that would be formed by this light cracking, is mixed in the tower H H'. No, excuse me. Is mixed in the upper part of the system with the straight run gasoline, which is undoubtedly much larger in amount, and since apparently this elimination of gum-forming constituents is due to polymerization, which means, to the best of our knowledge, the reaction together of two similar unsaturated constituents, I would expect that to take place much less rapidly when the unsaturated constituents were separated by relatively larger amounts of straight-run products.

The conditions are thus radically different from those of Lewis and Cooke, where you have a large body of gasoline, all of which is cracked, held in liquid state on successive plates of a tower under pressure.

Q. The gum-forming parts, if there were any, in Barbet would get into the heavy part of the kerosene, would it not?

A. Into the light part of the kerosene.

Q. I mean into the light part of the kerosene, instead of into the gasoline?

A. Yes, the gum-forming constituents which ordinarily bother us would probably go out in Barbet's kerosene; but I do not think any one would ever notice whether they were eliminated or not, because in the process of treating kerosene to make burning oils a much heavier treat is required than for gasoline, in order to get out other constituents than gum-forming constituents, and that would inevitably remove the gum-forming constituents long before you had sufficiently refined the kerosene to make a good marketable product. So that you would not know whether or not you had removed the gum-forming constituents in the kerosene by any action taking place in the tower.

Q. A man could run this for years, even if it had this function, and would never find it out?

A. That is correct.

Q. Because of the heavy treat that the kerosene would have to have anyhow?

[fol. 1291] A. And as I pointed out, these gum-forming constituents only deposit their gum on prolonged storage of the untreated material.

Q. Is there any specific disclosure in Barbet of any such function as that?

A. Not the slighest.

Q. So it must lurk somewhere in the necessary operation, if at all?

A. If at all.

Q. You were cross-examined, Mr. Wilson, at some length with reference to the Kittle process. I wish you would describe that process in detail and compare it with Burton and Ellis.

A. Exhibit 131, page 1642 shows the figure. Referring to Figure 1, on page 1642, the oil is supplied by the pump L to the supply tank A, which is stated to be held under a pressure of approximately 95 pounds per square inch. This oil is then forced through the regulating valve A^1 and through the long coil which is immersed in a lead bath, where it is stated in the patent that the oil is heated.

The Master: You have done this once before; you did it in your direct.

The Witness: No, I did not go into this in detail in direct, to discuss this operation. I mentioned one or two points.

[fol. 1292] A. (Continuing:) The oil is heated to about 932 degrees Fahrenheit. It is not stated exactly where that temperature is measured. In the first place, in passing through the valve there is undoubtedly a very marked loss in pressure, because the pressure in the other end of the system is only a matter of a few ounces, as I have shown. Much of the pressure is undoubtedly lost in passing through the valve A^1, and substantially all the rest of the pressure is lost by friction in this small diameter coil in the lead bath.

As the pressure drops and the temperature rises going through the coil, there is, of course, progressive vaporization, and only the very heaviest ends, probably mainly of polymerized constituents, would be left in the liquid state entering the drum, and that, as I pointed out, in the construction shown would run out immediately through the coil which must be partly filled with gas in order to maintain the pressure by means of the head of oil I-I, as in the vessel K^1, as Kittle indicates.

Therefore, in passing through that coil the velocity of motion speeds up rapidly as the pressure drops, and the

fluid vaporizes and expands, and of course the relative amounts of time that the material is in different parts of the apparatus is not proportional to their volume, because the pressure and temperature are varying, and that varies the speed with which you pass through different parts of the apparatus.

The drum F which is surrounded by lead at a temperature approximately 360 degrees Fahrenheit hotter than the oil, is so far as its function is concerned, is merely an enlargement or extension of the cracking coil, much as in Testelin and Rennard the final part of the pipe is larger in diameter and hotter than the first part of the pipe, and in this you have necessarily a very substantial rise in temperature, particularly since the vaporization has taken place largely before the oil came into this vessel, and a large portion of the cracking is therefore done in this hottest part of the apparatus, the enlarged part of the tube.

I want to point out, particularly, that while Kittle on account of having his enlargement buried in molten lead does not need to worry about the coke difficulties, his procedure of raising the temperature very markedly and putting a lot of heat into the enlarged section is just the opposite of what has been found necessary by Ellis and in the Cross and tube and tank processes if you want to use the tube and tank principle, which is fundamentally to have a large body of liquid held at a temperature slightly lower than [fol. 1293] the stream coming in, and to crack by holding a large body of liquid in there at this lower temperature with little or no fire.

If you attempted in one of those processes to raise the temperature to even a fraction of the extent that it would be raised in Kittle's drum, you would have to fire so hard that your coke troubles would be very, very serious and make an inoperative process. Kittle does not show at all the essential feature of producing the bulk of the cracking in a drum without rise in temperature, or without very considerable input of heat.

Q. Running as Kittle does, with relatively small pressure, a few ounces, as determined by this oil head, if he got a hot bottom it would not make any difference, would it?

A. No. That is another reason, in addition to its being in a lead bath, why hot bottoms do not need to concern him. He did not have that problem. The procedure which he

uses of raising the temperature markedly in the expanded part of the tube, making it thus really a part of his tube-cracking process, could not be successfully applied to a pressure process unless you had the enormous lead bath which would go with the commercial pressure process, and that would be not feasible. That would be another way of solving the hot bottom problem, but it is quite a different way, and it is a way which is not feasible on a large scale.

Q. Mr. Rauber asked you a question about the strength of a graphite tube, assuming 600 pounds tensile strength per inch. Was that answered?

A. No, I think he withdrew that question.

Q. Then I would like to have the answer to it, please. Assume a tube two inches internal diameter with one inch walls and 600 pounds tensile strength per square inch of cross-section. How much pressure would that carry?

A. That would theoretically carry a pressure in just equal to the tensile strength, a pressure of six hundred pounds per square inch in that size tube. Of course, one would not want to operate without a large factor of safety, possibly something like ten to one, so if that is the proper figure for the strength of graphite that would mean that you could reasonably operate at something like 60 pounds pressure in such a tube.

Q. In your cross-examination you were asked to point out where the feature of condensing under pressure in a [fol. 1294] gasoline-making pressure distillation process is shown in Adams—I am not sure that I have got the full question, but the point is clear enough—and you in your answer referred to figure 11 of the Adams patent No. 1,320,354. Is that also shown in patent No. 1,327,263?

A. Yes, it is also shown in that patent.

Q. Which of the two figures of that patent is it shown in?

A. In figure 1.

The Master: Which one is this?

Mr. Wiles: No. 1,327,263.

A. In figure 1 the vessel 32 is the receiving drum, the gas passing off through the small cock above the drum and at the outlet of the condenser.

Q. My question was not a full quotation, but I do not think that makes any difference. It is the second feature that you attributed to Adams that I had in mind.

You were asked in your cross about the amount of fuel which was burned around the Holmes-Manley still sections as compared with the amount burned under the pre-heater coil, and you answered, I think, that it was a fifth. Wasn't that what you said?

A. Yes, about a fifth of the fuel was burned around the stills.

Q. Now, what is the relative heat input in the preheater coil and into the drum?

A. The relative input of heat into the stills, as compared with the pre-heater coil and the economizer and the heat exchangers, is very, very small; by no means as large as would be indicated by this proportion of fuel burned because, in the first place, we use a large amount of excess air with the gas which we burn around the stills in order to prevent possible danger of overheating.

Secondly, because, as I pointed out, it is hard to get very much heat into a vertical still compared with a horizontal still.

And, third, because we allow the gases to go out from the still top at quite a high temperature. Then, you recall, that we pass them over into the economizer, which is set above the pre-heater, and pick up a considerable further amount of heat in that economizer.

As nearly as I can figure, only about seven or eight per cent of the total heat taken up by the oil is put in in the [fol. 1295] four vertical stills, the remainder being put in, either in the economizer, the pre-heater or the heat exchanger.

Q. You were cross-examined at a good deal of length about Pielsticker's column still W. I wish you would please summarize your views on that, briefly.

A. The description of Pielsticker's column still and of the place where it is put and the function which it performs are all very sketchy and vague.

As I pointed out, there is a definite inconsistency both in the British and American patents, between two of the statements regarding where the column still is placed.

Referring to the figure 4 on page 1667, one of four indications is that the vapors, after leaving the dome R on the still, pass into the base of the column through the pipe shown at the bottom on figure 10.

The other three statements in the American Patent indicate that the column still W is actually connected to the outlet of the pre-heating coil, and that the materials never pass through the ordinary still when the column still is used.

In other words, that the column still replaces the ordinary still. This latter view, which might be said to have a three to one probability, in view of the American patent, is further borne out by the fact that the lettering on the top part of the tower in the British patent——

Q. The tower W.

A. The tower W in the British patent correspond- exactly to the lettering on the dome and valve of the still in the same patent, indicating that they are alternative devices.

I therefore consider it substantially established that this tower is to go on the outlet of the coil, and then its function would be primarily to separate out entrained drops and to give some fractionation of the vapors leaving the coil, the tar plus any condensed material being withdrawn from the base of the tower by the steam ejector Z through the valve Y.

The construction of the tower is somewhat of a mystery. I pointed out the showing is of baffle plates in the part of the still that is broken away, and the question arises as to whether that continues all the way up, or whether there is some other kind of material in the upper part of the tower. So far as I can see it might be either.

With regard to the statement to which my attention was called on cross-examination, that in line 104, on page 1671, [fol. 1296] "The body of which column still I also divide by baffle plates X", I was not quite clear just why the term "also" was there. I think it probably——

The Master: I guess I am not looking at the right place.

The Witness: Line 104, page 1671. It says, "The body of which column still I also divide by baffle plates X" (indicating).

The Master: Oh, yes.

The Witness: I think the "also" very probably refers to the fact that he has previously spoken of using two different kinds of baffles in his still and of using baffles in the small tower, or the small stillhead R in Figure 5, which

he used in place of the ordinary stillhead. He seems to have a sort of a baffle hobby, and he is apparently saying, "I also divide my column still with baffles".

But, again, it is not perfectly clear just what he does mean by that. In any case, even if we should assume for the purpose of argument, that the column still was handling the vapors which come out of the still, which I do not think is the case and which is certainly not clearly disclosed, there is no indication of any return of the material to the body of oil. Any material which may be accidentally or purposely condensed in the column still W simply goes out with the tar and nothing is said as to what is done with it.

Q. Goes out through that steam ejector?

A. Goes out through that steam ejector which, of course, implies that it is a rather heavy tar rather than merely a distillate, such as it would be if it were attached to the still.

Recross-examination.

Questions by Mr. Rauber:

Q. You stated that in the Burton patent, No. 1049667, Burton referred to an antecedent distillation process.

A. Yes.

Q. That is the coking still process?

A. That would be either the coking, or the running for steam refined—both would answer the description, as far as given. Those are the two processes which make gasoline, kerosene, lubricants and wax, in common use in the industry.

[fol. 1297] Q. Will you tell me in a yes or no answer, whether any of the licensees using the Burton process, use charging stock made by any other processes than the coking still process, or the steam still process?

A. Within my knowledge, of course?

Q. Yes.

A. The answer, as of today, would be yes.

Q. That they do?

A. Use other processes than those two processes.

Q. Does the Chamberlain patent describe an antecedent process, too?

A. Yes.

Q. What is that antecedent process?

A. The coking still process, which he describes in detail.

Q. In flash cracking processes they used gas oil?

A. Yes.

Q. They used it as a clean stock, did they not?

A. As I said, I have no direct knowledge of that, yet I think it is quite possible that they preferred gas oil to residual stocks—residual fuel oil for example—because of greater cleanliness, though I don't know that of my own knowledge.

Q. What personal knowledge have you had of brown coal tars, such as those described by Krey and Riebech?

A. I don't know just what you mean by "such as those described."

Q. Well leave out the "such as." What is your personal experience, or what personal experience have you had with the brown coal tars described in the Riebech process?

A. Well, I haven't had any experience with the particular ones described in the Riebech process.

Q. Have you have any experience with the Baku petroleums, described in the Riebech process?

A. No, I have not.

Q. Have you *have* any experience with the Baku petroleums described in the Riebech process?

A. Not personal contact, no.

Q. What has been your experience in the cracking of California oils in Burton stills—have you had any actual personal experience with them?

A. I said I worked with gas oil from California crude oil, [fol. 1298] in a small way, at Whiting. I have also visited and discussed the cracking of California oils, with the Standard Oil Company of California and the Union Oil Company of California, and I have seen their cracking installation.

Q. At the time I asked you the boiling point range of materials heavier than gas oil, did you know what they were?

A. What do you mean?

Q. I asked you about what the boiling point of materials heavier than gas oil, was.

A. Well, I don't see how I could know an indefinite question like that. If you ask me about some particular material——

Q. I asked you about paraffin distillate, and you said you didn't know the boiling points, because they cracked.

A. No, I said we could not determine the boiling points at atmospheric pressure, because they cracked. You gave me no opportunity to say whether I could calculate them, or not, but I have explained, today, how I can calculate them.

Q. Can you give me some idea as to how the viscosity of gas oils and light lubricating oils would vary between 20 degrees Centigrade and a 100 degrees Fahrenheit?

A. Gas oil?

Q. Yes.

A. What kind of gas oil—petroleum?

Q. Yes.

Mr. Wiles: In what sense—American, or European?

Mr. Rauber: American—just a rough estimate.

A. I don't have any figures for gas oils. I have some figures for the lubricating oils.

Q. For light spindle oils?

A. I don't know whether I have any of that data with me, or not.

The Master: How much is 75 degrees Centigrade converted into Fahrenheit?

Mr. Rauber: 20 degrees Centigrade would be 68 degrees Fahrenheit.

Mr. Wiles: The change would be from 68 to a 100, Fahrenheit. That is the question, is it not?

The Witness: I have some curves for the oils above kerosene, but they only go to about a hundred degrees, or a little lower. I have no curves as low as 60 degrees, and, of course, my curves only apply to mid-continent oils. They are different for different kinds of oils.

[fol. 1299] Q. How much personal experience have you had with shale oils?

A. Well, I have had quite a number of samples come in,—I suppose four or five—and we have tried experimental cracking of them, and I have also made it a point, a couple of times, to go to the Bureau of Mines, and discuss with them the general status of the industry.

Q. I was only asking for your personal experience, aside from discussions.

A. I see. I have also tested a number of shale products—both gasoline and lubricating oils—that have been sent us from time to time.

Q. Did you ever actually crack any shale oils?

A. Yes.

Q. When did you do that—how long ago?

A. About two years ago.

Q. Where?

A. At Whiting.

Q. You have given the lowest temperature of cracking at about 625?

A. For petroleum oils.

Q. Do you know whether any others have found lower temperatures?

A. No. Some may have, on some particular oils, I presume.

Q. You have never seen any publication of any lower temperatures?

A. Not that I recall.

Q. Now, in that test to determine the effect of condensing under pressure and the losses in gasoline, you did not run two parallel tests, in one of which you condensed under pressure and in another in which you condensed under atmospheric pressure?

A. We did that, too. That was not the method which I described, but we did that as well, but we don't consider that as accurate as the one which I described.

Q. What is the heat of vaporization of material at its critical temperature?

A. The heat of vaporization is zero at its critical temperature—theoretically.

Q. Now, at these critical temperatures of gasoline in contact with a heavy hydrocarbon material, is all the gasoline absorbed in that heavy material?

[fol. 1300] A. No, I shouldn't say so. I suppose there might be conditions under which substantially all would be absorbed.

Q. In case there was no atmosphere above?

A. In case your pressure was extremely high.

Q. And you had an atmosphere, or vapors above?

A. Even then it might be substantially all absorbed. It cannot be all absorbed. There are always some molecules in the vapor space—if they are present in the liquid space, and equilibrium has been attained.

Q. You described the refining of petroleum in pipe stills—what products do they get in that refining?

A. Well, we apply pipe stills to many different stages of refining.

Q. I am speaking of crude—applying it to a crude oil.

A. Well, again it depends on the type of crude we start with. Our main use of pipe stills on crude, at the present time, is on quite heavy crudes, which contain very little light ends, and we are working mainly to get heavy lubricants off.

Q. What type of lubricants are they—what are the names of them?

A. They are lubricants that we use in making our motor oils.

Q. Cylinder oils?

A. No.

Q. You don't get any cylinder oils in that process?

A. No.

Q. Now, in the Burton patent No. 1,448,254, the process described in that patent, does any of the——

A. Pardon me—Exhibit number, please?

Q. Exhibit 195.

A. That is the Burton and Clark.

Q. Does any of the heavy liquid in the drum 10 go down through the pipe 32 and into the coil 10?

A. Yes. I pointed that out as being the defect in this process, why it did not make as much improvement as might be desired.

Q. And that that goes down, mixes with the reflux from the pipe 16?

A. Yes.

Q. Did you say that Ellis used or described a clean stock in his patent No. 1,415,323, the tube and tank patent?

A. What was your question, did I say or would I?

Q. Does he describe any clean stock?

A. You had better change the question.

[fol. 1301] Q. Yes, change that question to does he describe any clean stock?

A. Ellis patent number?

Q. Ellis patent No. 1,415,323, Exhibit 192.

A. I do not think he, in this patent, specifically describes his oils except as heavy oils.

Q. What is the fire test on water-white kerosene?

A. Why, generally around 190 Fahrenheit.

Q. Do you sell any at 150?

A. No.

Q. Kerosene having 150 fire test would be a lighter product than that with 190 test.

A. Yes.

Q. Now, what is the temperature at which polymerization takes place in the Lewis bubble tower?

A. I cannot tell you just where in that tower the polymerization takes place. I can give you the approximate range of temperatures, and we know that it takes place some place in that range, probably over most of the range.

Q. What is the range?

A. The top of the bubble tower under normal operations of a Burton still at 95 pounds will be approximately 510 to—well, we will say 500 to 525 Fahrenheit; the temperature at the bottom of the tower is in the neighborhood of 700 Fahrenheit.

Q. Have you personally carried out any experiments on the critical temperature of the oils or hydrocarbon products?

A. No, I have not.

Q. When you are heating an oil passing through a coil, immersed in a hot bath and you wish to raise the temperature of the oil at any zone in that coil, to raise it sharply, would you enlarge the diameter of the coil at that point?

A. Just what do you mean by "raising it sharply"?

Q. Well, raising the temperature at that point more than at any other point?

A. I do not see how I could. If I had a coil in a bath my rise in temperature would, of course, gradually decrease as I approached the temperature of the bath.

Q. Your rate of rise of temperature would not decrease as you approached the temperature of the bath?

A. The rate of rise would decrease as I approached the temperature of the bath.

Q. If you had an enlargement in the coil at any point, would that increase the rise in temperature or decrease the rate of rise?

[fol. 1302] A. Are you talking about per unit time?

Q. I mean in volume of material?

A. No, I should not say that would speed up the rise.

Q. It would not have a tendency to increase the normal rate of rise of temperature, would it, in that coil?

A. No.

Q. It would have a tendency to decrease it, would it not?

A. Depending on the ratio of surface to volume of the products therein.

Q. And if you enlarged the coil, in that enlargement the ratio of surface to volume would decrease, would it not?

A. If you enlarged it in the form of a cylinder, but you might enlarge it in the form of some flat object, in which case the opposite would take place.

Q. I mean in the form of a cylinder.

A. If you enlarged it in the form of a cylinder you would cut down the heating surface per unit volume.

Q. And in that way you would cut down the tendency for its temperature to rise?

A. But you would keep it in that much longer, which would tend to work in the other direction, of course.

Q. Yes. How does the tensile strength of graphite compare with that of steel?

A. I do not know. I tried all day to find out what its tensile strength was.

Q. Do you know the tensile strength of steel, approximately?

A. Oh, the tensile strength of steel at ordinary temperatures is around—that is, the ordinary mild steel which we use——

Q. I just mean approximately.

A. It is around 55,000 pounds. I cannot be sure within five thousand.

Q. In that kerosene of 190 fire test do you have much or any hydrocarbons that would normally go into the gas oil, or could go into gas oil?

A. In our present-day kerosene, on account of lacking the light ends, we have comparatively little of the heavy ends, if we had——

Q. Comparatively little of the material that might be classed as gas oil?

A. Yes.

WARREN KENDALL LEWIS, a witness called on behalf of the defendants, being first duly sworn, testified as follows:

Direct examination.

Questions by Mr. Wiles:

I am a graduate of the Massachusetts Institute of Technology in its course in chemical engineering. After graduation I worked in the laboratories of that institute for a year and then studied abroad for two years, taking my degree in physical chemistry in the University of Breslau; taking my examination under Dr. Ladenburg, the head of the department.

On returning to this country, I worked first in the laboratories of the Institute, later went out with the W. H. McElwain Company, in leather work, returning to the Institute as a faculty member in industrial chemistry in 1910, and have been on the faculty ever since.

I am now the head of the department of chemical engineering in that institution. I have done outside consulting work and in the department which I head are two divisions, our research laboratory of applied chemistry and our school of engineering practice, which have a great many contacts with industrial concerns and in connection with which I have done a great deal of work, research and otherwise, along various industrial lines.

I have specialized particularly in the field of distillation, in that connection doing a good deal of work in the oil industry in connection with the Standard Oil Company of New Jersey and others.

Q. There are a number of statements with regard to physical, chemical and engineering facts made by the plaintiff's expert bearing on this case about which I want to question you Dr. Lewis, and if you have not already referred to the record on each of these points in order to satisfy yourself

[fol. 1303] as to the context, I wish you would do so as I ask you the questions.

On page 643, near the middle, Dr. McKee stated with regard to kerosene, "The flashpoint would be fairly close to the boiling point." Is that statement correct?

A. No, that statement is not correct. The flashpoint of a kerosene is roughly 200 degrees Fahrenheit below the boiling point.

Q. On page 664 plaintiff's expert calculated that the vapor pressure of hexane at its flashpoint would be about 1/16th of an atmosphere, is that correct?

A. No, that statement is incorrect. The flashpoint of hexane is approximately 20 to 25 degrees below zero Fahrenheit, at which temperature the vapor pressure is somewhere between 1/70th to 1/100th atmosphere and not a 16th of an atmosphere as stated by the plaintiff's expert.

Q. On page 679, plaintiff's expert said that compressing vapors before condensation aided condensation because it heated the vapors; is that correct?

A. No, I think that is a misinterpretation of the situation. Raising the pressure on the vapors raises the boiling point or condensation point of those condensable fractions in the vapors, and therefore, raises the temperature at which those vapors will come down. It therefore makes it easier to condense the vapors because the temperature difference is greater, and a smaller condenser can be employed or a higher temperature of cooling medium.

On the other hand, compression, inasmuch as it raises the temperatures of the vapors, means more heat must ultimately be extracted, inasmuch as, in general, it is necessary not merely to condense the vapors but also to cool them to substantially the same temperatures, whether the condensation be under pressure or not.

Q. On page 980, plaintiff's expert states that hydrogen and methane are but very slightly soluble in oil, and to prove his point with regard to methane, he cites the fact that in natural gas wells undissolved methane sometimes comes out at pressures of a thousand pounds. Please state what, if anything, this proves as to the solubility of methane in natural gas and also what the facts are?

A. The fact that methane does escape from oil occasionally under pressures as high as a thousand pounds proves nothing about the solubility except that there is some solu-

bility. On the other hand, this solubility may be low or [fol. 1304] may be high. No information is conveyed as to the extent of the solubility by this escape at a thousand pounds, except the fact that at the point where the oil originated there was enough methane to saturate that oil and put in an excess over and above that which the oil can hold at a thousand pounds.

Now, as to the facts: The solubility of methane in various crude oils has recently been determined by the Bureau of Mines and has been found to be approximately 45 to 50 per cent by volume at ordinary temperatures, 70 degrees Fahrenheit, at one atmosphere pressure. Therefore, at a thousand pounds pressure the solubility would be, roughly, 30 parts by volume for each volume of oil, a very considerable solubility.

Q. Plaintiff's expert at several points criticised the practice of the petroleum industry in the design and use of fractionating towers. I want to ask you some questions on those points.

In the first place, he said on page 693 that the reflux liquid for a fractionating tower, such as those commonly used in the petroleum industry, should not be put in on the top plate but three or four plates below the top; is that true?

A. No. Over thirty years ago the German expert, Hausbrand, in a book on the design of rectifying equipment gave a carefully reasoned demonstration of the fact that the reflux should be put upon the top plate of a rectifying column. Furthermore, I have never seen this demonstration of Hausbrand questioned or refuted, and I have never heard of anyone's questioning or trying to refute it.

As the result of those points brought out by Hausbrand, the distillation industry rapidly began to discontinue the practice frequently employed before that time of returning parts of the reflux at lower points in the column, and in recent years this practice of doing what Hausbrand demonstrated should be done, namely, putting the reflux in on the top plate, has become almost universal practice.

Q. Plaintiff's expert also stated that the reflux is, in practice, ordinarily returned to the third or fourth plate of a fractionating tower, and when he was asked to give an example of such practice in the oil industry, he excused his inability to do so by saying that the oil industry is very

much behind in its use of bubble towers and refluxes. What have you to say on that?

A. Inasmuch as the oil industry very generally returns its reflux on the top plate, that point shows that the oil in- [fol. 1305] dustry is familiar with good practice in the rectification industries, as illustrated by such authorities as Hausbrand and others.

Q. He also stated on page 578 that an air-cooled bubble tower on a pressure still could not possibly operate well or operate successfully to give a well-fractioned pressure distillate without having a dephlegmator at its upper end. What have you to say about that?

A. As a matter of fact, rectifying columns can be operated entirely successfully in the petroleum industry by air cooling. For this there are three reasons.

In the first place, the temperature at which these columns are ordinarily operated for the operations for which they are ordinarily employed is very high. Consequently, the rate of dissipation of heat from the top of the column is very great. Consequently, you are producing in that column itself a reflux; the column itself is acting as a dephlegmator as well as a rectifying column.

In the second place, in the petroleum industry the reflux ratios required for the ordinary operations to which columns are put—that is to say, the separation between gasoline and kerosene cuts and between kerosene and the gas oil cuts—the reflux ratios required are surprisingly low, and consquently a great deal of dissipation of heat is not necessary. It is a great deal less than is necessary, for example, in the old alcohol art.

Finally, the heat of vaporization of these materials that are being condensed is low, and in consequence the amount of heat necessary to dissipate to the air in order to produce the reflux required is small; and because of those facts it is perfectly practicable to work these columns air-cooled.

While it is practicable to do this, there are certain advantages in putting the dephlegmator on top of the column. In the first place, it makes it possible to economize heat and, in the second place, it makes it possible to get a little bit more effectiveness out of the column itself. But it does require additional equipment, and the decision as to whether it is worth while to put on dephlegmators or to use an air-cooled column is a matter of engineering judgment.

One engineer might wish to use an air-cooled column and another to use the dephlegmator on top instead. It is a question of dollars and cents cost, and is influenced by a great many factors in the design of the rectifying equipment.

[fol. 1306] The Master: The dephlegmator, then, would simply carry the fractionation a step farther?

The Witness: No, you can get all the fractionation by the use of an air-cooled column that it is possible to get by the use of a column plus a dephlegmator, where the temperatures necessary for the condensation are high. Of course, if the temperatures are low you cannot use air cooling and get the results, but where those temperatures are high it is possible to get all the separation in the column that it is with the column plus the dephlegmator. The column in that case has a twofold function: It serves both as a dephlegmator and column.

Since it is doing a twofold job, obviously it has to be larger, either taller or larger in cross-section and general bulk. But it is frequently simpler to build a larger column than to go to the complication of putting a separate unit of apparatus, namely, the dephlegmator, on top of that column. It is a matter on which engineers are divided as to advisability, but there can be no question as to the operability and the practicability of both schemes.

Q. Plaintiff's expert stated or indicated on page 448 that a column filled with Raschig rings was better from an engineering and chemical point of view than a plate and cap type of tower for this kind of work. Do you agree with that?

A. No, I cannot agree with that. Here again we are dealing with a question of engineering advisability. It is possible by using tower filling of the Raschig type, very small in size, to get excellent efficiency and to construct a column that is just as simple and just as effective as a bubble cap column, provided other conditions are met to which I will refer in a moment.

On the other hand, such filling is very expensive. If one wishes to use larger filling, which is cheap, and cheaper than bubble cap construction, one loses this efficiency. If one wants to get the efficiency one must use expensive fill-

ing. But with the most effective Raschig ring type of filling, despite the fact that it is expensive in large columns, such as are used in the petroleum industry, one runs into difficulty that is practically insuperable, and that is channeling. In other words, in such a column the vapors have a tendency to go up one side of the column and the liquid to come down the other. The whole purpose of these columns is to get effective contact and interaction of vapor and [fol. 1307] liquid, and if the vapor goes up one side of the column and the liquid comes down the other the whole purpose of the equipment is lost. If the vapor does once start up one side and the liquid down the other, the vapor finds a free, unimpeded path on its side and the liquid finds a free, unimpeded path down its side, and there is no tendency for the column operation to adjust itself.

The beauty about a bubble cap column is that it compels the vapor to go through the successive pools of liquid on the successive plates of the column, and the danger of channeling, especially in large columns, is so great that I cannot agree that Raschig rings can compete in efficiency and effectiveness with bubble plate columns; and, as a matter of fact, the distillation industry is certainly in my opinion sweeping away from the use of these filled types of columns and going over toward the use of bubble caps, except for very special cases.

Q. On page 1095 and 1097, plaintiff's expert said that in an apparatus with red hot walls somewhat coated with carbon the temperature of a stream of oil flowing through it would be ten per cent lower than that of the walls. What have you to say on that?

A. Well, the temperature of the oil flowing through such a red hot pipe might be almost anything from room temperature up. The temperature difference which it is possible to maintain between a body of liquid—or gas or vapor, for that matter—flowing through a pipe, and that pipe, depends on the velocity of the liquid and a large number of factors, so that the fact that the pipe is red hot proves nothing whatever as to the temperature of the liquid inside that pipe. It is possible to have quite cold liquid inside a red hot tube—all the more when it is understood that there is a certain layer of carbon on this tube because the carbon itself is a poor conductor. But the main thing that offers resistance to flow of heat from the red hot tube to the oil,

and prevents a cold oil from cooling the pipe as rapidly as it might, is the insulating action of that little layer of oil itself right next the tube. That is very great, and the temperature difference can be very large indeed.

If the oil is run at a high velocity so as to scrape off that surface, and if the oil is given sufficient time to come up to the temperature of the tube, of course it will tend to come up. In this case it could not get up to red hot temperature without cracking intervening.

[fol. 1308] The Master: When you said you might have a quite cold stream of oil through a red hot tube, you mean relatively cold, did you not?

The Witness: Cold enough so you can put your hand in it. The point I am trying to make is that the conditions were very indefinite. I can have a red hot tube and send a stream of oil through it at substantially room temperature. I can have the same red hot tube and by adjusting my conditions send a stream of oil through it so that it is very hot. Of course, in the case of oil I could not get it up to high temperature and still have an oil, because it would decompose long before it got there.

Q. Plaintiff's expert said on page 1098, that there would be marked variations of the temperatures of different points of a lead bath approximately a foot in diameter and two feet deep, and when pressed for justification of his statements, he said he had tested other baths showing variation of 50 to a hundred degrees in different parts. On further questioning it appeared that the baths in which he found these variations that he spoke of, were air baths, instead of lead baths. What have you to say as to his deductions, and as to the facts?

A. One would conclude from this lack of uniformity of temperature in air baths, that lead baths will be quite uniform in temperature. As a matter of fact, lead baths are very uniform in temperature. The reason is twofold. In the first place, molten lead has a heat conductivity about a hundred times as great as that of air. In the second place, lead has a density—my recollection is that it is fifteen thousand times that of air. Now, its specific heat is low. But on the other hand, if a given volume of lead comes up to the surface of the thing being heated, that volume of lead brings up two thousand fold as much heat as the same vol-

ume of air coming up to an equal proximity, and consequently the capacity of lead to hold a body that is being heated in it, at substantially uniform temperature, is a hundredfold—perhaps a thousandfold as great as that of air.

Now, as a matter of fact, I have used lead baths myself. For example, a few years ago I had a piece of research work in which it was necessary to hold a constant temperature, at a high temperature level, and I employed a lead bath for that purpose, and it worked beautifully. We anticipated practically no temperature differences, and we never [fol. 1309] found temperature differences exceeding a fraction of a degree, and that was approximately the precision at which we could measure such temperatures, at that high temperature level.

Q. On page 338 of the Record I find the following questions by counsel, and answers by the plaintiff's expert:

"Question. You find in American petroleums largely straight chain hydrocarbons. Is that what you mean? Answer. Yes, though some American petroleums we do find some ring compounds, but only comparatively few."

Is that testimony true?

A. That is a misstatement of fact. The evidence is uncontrovertible that we have no American petroleums in which the major portion consists of straight chain hydrocarbons. The work of Mabery, in recent years, has shown that the highest boiling fractions—the lubricant fractions of American petroleums, other than the wax, which consists of 5 to 10 per cent of those fractions, and which is removed by the pressing operation, to which references have already been made in the testimony—that all the other higher boiling fractions are not straight chain saturated hydrocarbons, but are apparently ring hydrocarbons, which do not belong to the straight chain series, at all, which do not crystalize as waxes, but which have value as lubricants.

In the lower boiling fractions of American petroleum the evidence is not quite so conclusive as those obtained by Mabery for the higher boiling fractions, but Wagner has shown that in the lower boiling fractions the predominating constituents are apparently ring compounds, compounds containing less hydrogen than the saturated straight chains, and not belonging to the straight chain paraffin series.

When you get down to the lower ends, the lower, more volatile fractions of gasoline—there you find a higher percentage of these straight chain hydrocarbons, but even there not—by any manner of means, all of the hydrocarbons are of these paraffin series.

But in the old days, when we first began to study petroleum, it was easier to isolate these low boiling fractions and these straight chain hydrocarbons were found in those and, consequently, they gave their name to the whole crude, but we know, today, that the crude does not consist, predominatingly of those materials.

Q. Practically everything that did not dissolve in sulphuric acid was called paraffin, wasn't it, in the old days? [fol. 1310] A. In the old days, yes.

Q. Plaintiff's expert also said, on page 667, that paraffin hydrocarbons may be made of almost all Pennsylvania crude oils; and, further, on page 668, that he believed Pennsylvania crude oil had not been shown to contain more than one or two per cent of ring-compound, and that although some people had stated that there was 20 per cent or more, he did not think that they had proved their case.

When pressed to account for the fact that Pennsylvania crude oils gave large yields of high boiling lubricants, which failed to give the high melting points characteristic of paraffin hydrocarbons, he attempted to explain this, on page 670, by saying that although the individual paraffin hydrocarbons present might have high melting points, the mixture of several consecutive members of the paraffin group would have a lower melting point than that of the individual hydrocarbons.

What is the fact about that?

A. So far as I know there isn't one bit of evidence to indicate that a mixture of two straight chain hydrocarbons ever melts at a lower temperature than the components of which it is composed.

The data indicates that these mixtures always melt at intermediate temperatures. Now, when we mix two materials of varying melting points, and get the melting point of the mixture, we encounter a number of different phenomena, but two important types of phenomena.

We may form a eutectics mixture, which melts at lower temperatures than either of the components, and that, I

presume, is the assumption that the Plaintiff's expert made, when he made the statements referred to in the question.

On the other hand, we also have exceedingly important cases in which, when we mix two components, the melting point of the mixture is always between that of the two components.

For example, if I took silver and gold and added gold to silver in an amount however small, the melting point of the silver is always raised—always raised.

The data all indicates that paraffin waxes behave in the same way. They form so-called solid solutions, one in the other, and the melting point of the mixtures lies in between the melting points of the individual components.

Now, the evidence, here is not so absolutely conclusive as one would wish, the reason being that it is very difficult to [fol. 1311] prepare absolutely pure paraffin hydrocarbons, with which to experiment. But all the data indicates that the phenomenon is one of solid solution, that the melting point is intermediate between the melting points of the components, as I have stated.

Q. Now, I wish you would state a little further your views, or the facts on the composition of these Pennsylvania lubricants which plaintiff's expert said were not solid, because they were mixtures of hydrocarbons. What are they, really?

A. Instead of these Pennsylvania lubricants being mixtures of paraffin hydrocarbons—complex mixtures, which do not freeze at low temperatures, because of the depressing action of each on the freezing point of the other—the facts are quite the reverse.

Mabery produced from characteristic Pennsylvania crudes these lubricating stocks. He then fractioned those lubricating stocks into narrow cuts, which were, perhaps not pure hydrocarbons, but, at least, consisted of closely related hydrocarbons, and then he analyzed these hydrocarbons.

Now, we can tell the relations between the hydrogen and carbon in any of these hydrocarbons of a given series, the minute we know its molecular weight. He analyzed them, and he always found a deficiency of hydrogen, and, as I recollect it, his lowest was six hydrogen atom molecules, and his highest was up to twelve or fourteen—or more, perhaps

—and not once, in that fraction, was he able to detect a fraction of straight chain paraffin hydrocarbon.

Q. On this same point of the lowering of the melting point of wax, I want to call your attention to the still more explicit statement of the plaintiff's expert on page 649 to the effect that if two pure paraffin hydrocarbons are mixed in approximately equal proportions, the melting point of the mixture would be lower than the melting point of either constituents, and as an example, he said that the melting of an equal mixture of a hydrocarbon melting at 42 Fahrenheit and one melting at 72.5 Fahrenheit would be about 32 Fahrenheit; is that true?

A. No. I am convinced that that is not true, but I have not been able to check that matter up with really pure hydrocarbons of that melting point and of really known purity, but all the evidence indicates that that would not be [fol. 1312] true. The melting point of the mixture would be intermediate between the melting point of the constituents.

Q. Tell us about what work you have done on waxes to determine what evidence you have in mind?

A. For example. Holde, one of the recognized authorities on petroleum products, states that the melting point of wax mixtures is not only intermediate between the melting points of the constituents, but the relationship is linear between the two.

Lewkowitch gives data showing when you fractionate waxes you always split up a wax of a given melting point into waxes of lower melting points and higher melting points, and when you mix those two together again you can synthesize the original waxes, and similar fractionations have been conducted in our own laboratories to substantiate Lewkowitch's data.

Q. When you say fractionate, you mean fractionate by distillation?

A. Most of these fractionations have been conducted by fractional crystallization, but some of them I think by distillation.

Q. Have you personally worked on the determination of the iodine and bromine numbers of various hydrocarbons?

A. I have done quite a little work on iodine and bromine numbers of fats, and on one particular hydrocarbon in which I am very much interested, namely, the polymerization product of isoprene, rubber. I have followed the cur-

rent literature on this question of unsaturation because I am very much interested in it.

Q. On page 658 and elsewhere, plaintiff's expert said that a ratio of 1.6 between the iodine numbers and the bromine numbers of hydrocarbons was generally accepted by chemists, though it might not be quite accurate for aromatic hydrocarbons. Is that statement of his true?

A. No, this statement cannot be accepted as dependable. It is well recognized by chemists that while this ratio 1.6 holds for a great many pure compounds and for a great many fatty compounds that there are a great many cases where it does not hold at all, and particularly in connection with hydrocarbons.

Q. On this, further, I want to call your attention to a considerable number of statements by plaintiff's expert, regarding the iodine number and unsaturation of benzol and [fol. 1313] similar aromatic hydrocarbons. On pages 348 and 349, which was near the beginning of his testimony, he said that benzol and toluol were unsaturated hydrocarbons, and that three tests were commonly used to distinguish between saturated and unsaturated hydrocarbons, namely, sulphuric acid absorption, iodine number and bromine number; unsaturated hydrocarbons responding to all three tests and saturated hydrocarbons not responding to any of them.

In discussing the Bassett and Boynton references, on page 355, he seems clearly to imply that benzol and similar coal tar distillation are unsaturated and have high iodine numbers, so that they come within one of the patents here, the Lewis patent.

Further, on page 356 he says that a product that Williams described was highly unsaturated because it contained large percentages of unsaturated hydrocarbons, such as benzol and toluol, but he states that he cannot calculate the exact iodine number because he does not know what other unsaturated compounds were present.

Later, on cross-examination, page 656, he again says that benzol and similar aromatic ring compounds are unsaturated and that they respond to both the bromine and the iodine tests. He then proceeded to calculate the iodine number due to the presence of benzol in a saturated naphtha.

Again, on page 659, he stated that the bromine number of pure benzol was about 36, but when asked to calculate the

iodine number, he expressed some doubt as to the readiness with which iodine reacted with benzol.

On page 665 he calculated that the bromine number of benzol would be 165. He was then asked to determine the bromine and iodine number of benzol and its mixtures with petroleum naphtha, and he reported on page 791, that actually they were negligibly small for pure benzol and that the addition of one part of benzol to five parts of 70-degree naphtha decreased rather than increased the bromine and iodine numbers of the naphtha; apparently contrary to his calculation on page 657.

Finally, in his re-direct examination, on page 1135 he says that benzol is well known to react with bromine and iodine with considerable difficulty, and indeed scarcely at all.

Now, I want you to state which of these statements are true with reference to the iodine and bromine numbers of benzol?

[fol. 1314] A. The last one is the true statement. Iodine and bromine react with benzol, under conditions of determination of the iodine and bromine numbers with difficulty, if at all.

Q. On pages 625 and 627 Dr. McKee at several points indicated that cylinder oils were generally or at least frequently distilled from crude, is that true?

A. No. So far as my contact with the industry is concerned, cylinder oil is a term used to describe a residuum from high-grade lubricating crudes left on distilling out the lighter fractions, including the lighter lubricants; and so far as I know, the term cylinder oil in the trade is not employed to designate a distillate.

I think that if the term cylinder oil were used to designate a distillate, it would be necessary to modify the term in some way, otherwise it would be absolutely misleading to anyone familiar with ordinary distillation practice in petroleum.

Q. In the course of his testimony the plaintiff's expert had occasion to discuss the following description of an oil, and the one I am calling your attention to, being in the translation of the Riebech German patent No. 37,728, Exhibit 97, which is as follows:

"When 25 to 30 per cent of distillate is drawn off there remains a residue having a viscosity of 7 to 10; a specific

gravity of 930 to 950 and forming a profitable lubricating oil of good quality."

Now, what would you understand was the gravity-viscosity relationship of these oils?

A. Well, the viscosity must fall between these limits of 7 to 10, and the specific gravity between the limits of .93 to .95. But my interpretation would be that he might have an oil with a viscosity as low as seven and a gravity—such an oil would have a gravity of around 93. He might have another oil with a viscosity as high as ten and it would probably have a gravity of around 95.

Q. That would not mean to you, would it, an oil of a gravity of 93 with a viscosity of ten, or a gravity of .95 with a viscosity of seven?

A. Certainly that would not be the interpretation that I would put on it.

Q. And when you found similar series, similar descriptions of other oils, you would, as one skilled in the art, assume that the lighter oil had the lower viscosity and the heavier oil the higher viscosity and not vice versa?

A. I would always assume that for a given series, because those are the general relationships for a given series of hydrocarbon compounds.

[fol. 1315] Cross-examination.

Questions by Mr. Rauber:

Q. Dr. Lewis, you stated that the solubility of methane in crude oil was 45 to 50 per cent by volume at atmospheric pressure. What would that be in percentage; what would that be in weight? What weight of methane would be dissolved in crude oil?

A. Well, under the thousand pounds pressure referred to by Dr. McKee that is approximately two per cent.

Q. And under the 45 to 50, which is what I asked you——

A. Under the one atmosphere pressure——

Q. Yes, what per cent?

A. That would be about 2 divided by 68; 3/100ths of one per cent.

Q. Is the solubility of methane in crude oil small as compared with the solubility of carbondioxide in water?

A. My recollection of the solubility of carbondioxide in water is that it is about volume for volume, which would

mean that the solubility of methane in crude oil would be half as high as that of carbondioxide—CO2 in water. But now, that figure for CO2, and water is memory only.

Q. In connection with the positioning of a refluex in a fractionating column, did they ever put baffle plates above the dephlegmator in a fractionating column?

A. Baffle plates above the dephlegmator?

Q. Yes, any kind of plates above the dephlegmator or cooling coil?

A. To knock down droplets out of the noncondensed gas?

Q. I do not care what the purpose is. I just asked you do they?

A. Well, I have never—in a rectifying column I do not remember ever seeing baffle plates above the dephlegmator of a rectifying column.

Q. You are familiar with Robinson's book on distillation, are you not?

A. Reasonably so.

Q. Does he not show the use of a cooling coil below some of the top plates, or rather, the reflux comes back on the second or third plates from the top?

A. He may.

Q. At least he would consider that fairly good practice then, would he not, if he showed it?

A. If he showed it? I do not know, I would want to see what he said about it.

Q. You are not familiar enough with the book to know just offhand?

A. No, I do not remember that he showed that. He may have showed it, you know, to demonstrate the old practice; the old practice in the art that was general, common practice and it has only slowly died out.

Q. Hausbrand's work did not refer to oils, did it? Hausbrand's book was on distillation in general?

A. Hausbrand used as his illustrative material throughout alcohol and water, but now the general applicability of the technique of alcohol distillation to the distillation of hydrocarbons has been recognized for over seventy-five years.

Q. When did they begin to extensively use the fractionating columns in oil distillation—that is, in commercial work?

A. They have been used abroad for 25 or 30 years.

Q. How long in this country?

A. In this country their extensive use has been more recent; apparently for the last ten or twelve years.

Q. How long have they been used in the alcohol industry?

A. Oh, 100 years, 500, something of that sort.

Q. How long in the benzol industry?

A. The first man to recommend their introduction into the benzol industry did so in, I think it was, 1849.

Q. In using rectifying columns, you would not get as good a rectification with air-cooling, as you would cooling at the top and preventing radiation throughout the length of the tower, would you?

A. I understand we are now, for instance, cutting say between gasoline and burning oils—such a case as cutting between gasoline and burning oils. On the assumption that we are cutting between gasoline and burning oils, or higher distillates, we can run an air-cooled column and get [fol. 1316] just as good rectification as we can by the use of the dephlegmator. You will have to use a different type of column, you will have to use a taller column; you will have to run that column properly. But, on the other hand, the column air-cooled will give just exactly as good results as a column with a dephlegmator. I do not say that it will give as good results as the same column plus a dephlegmator; but, designed to get that result, you can get your result in both cases and operate practicably and workably.

Q. In that case would you insulate the lower part of the tower at all, where you use the air cooling?

A. If you want to. It is optional.

Q. It is largely a matter of judgment whether you want to air-cooling, insulate your tower and cool at the top?

A. It is a matter of engineering judgment. There are very decided advantages in using a dephlegmator on top of the column. There are certain obvious simplifications in getting rid of that apparatus, with the control that is necessary in it.

Q. Whether you use Raschig rings or plates and caps depends largely on conditions and judgment and one thing and another like that?

A. Again, that is a matter largely of judgment, but it is my conviction that the judgment of designing experts in

the rectification field is swinging toward the bubble plate cap as the best practice under general conditions.

Q. For large bodies of material?

A. Especially for large towers.

Q. Yes. You said you might pass liquid through a red hot pipe and still have it come out quite cool. You would use a short pipe in that instance, would you not?

A. Oh, it does not have to be short. Of course, it must not be too long or you would get the liquid hot. It is surprising how long a pipe you can have and still shoot through the middle of it a liquid which is relatively cold. And you want to remember that the velocity of the liquid comes in the middle, so that the bulk of the liquid comes in the middle, and that the average temperature of the liquid coming up through can be and frequently is surprisingly low.

Q. If you had a pipe of small diameter and lots of bends in it you would find it more difficult to pass that through?

A. You would find it more difficult.

The Master: Without heating?

[fol. 1317] The Witness: Without getting hot.

You would find it more difficult getting it through, too. It would take more pressure to get it through.

Q. In connection with that lead bath at constant temperature, you had that bath insulated, didn't you, or lagged?

A. We had it fired. The upper side of it was insulated, but the bottom side of it was exposed to the direct flame.

Q. You would get more variation if you did not have it insulated, would you not, on the sides?

A. Well, you would get more, but we got a fraction of a degree, and maybe we would get a whole degree if we did not insulate it. Essentially it is impossible to get a large temperature variation in a bath of hot lead. If the top of the lead gets cold anywhere, it falls; you get a thermo-siphon circulation, and the lighter lead from the bottom rises. You get rapid circulation and rapid equalization of temperature.

Q. It is the rapid circulation rather than the thermo conductivity that gives you uniform temperature?

A. No, if the temperature is the same, you get just as rapid circulation in air as you would get in lead. Roughly, you get better circulation in air than you do in lead, but

when you move a certain volume of lead you are moving an enormously larger weight and a much larger thermal capacity. Its heat carrying capacity is greater. And, furthermore, when you move this large volume of lead with its high thermal capacity, it obviously is able to give up heat to any body that is soaking up heat from the lead. Its thermal conductivity is an essential factor, a very essential factor, in this uniformity of temperature.

Q. Wasn't it considered for a long time that Pennsylvania crudes were made up of straight chain hydrocarbons?

A. Yes, I think that even as late as 1906 in Mabery's article of 1906 he felt there was still a goodly percentage of straight chain hydrocarbons in Pennsylvania crude; but since the publishing of his article of, I think it is, 1923, that position has been absolutely untenable, and I think the experts in the industry have agreed that that position was wrong for the last ten or fifteen years, although it was only the publishing of Mabery's article that put into the literature the proof of it.

Q. What books has that appeared in other than journals?

The Master: What book has what appeared in?

Mr. Rauber: These articles of Mabery's showing that Pennsylvania oil contains a great deal of straight chain hydrocarbons.

A. I have not any record of their appearing in a book, except the Journal of Industrial and Engineering Chemistry of the American Chemical Society, the standard journal on chemical and chemical-engineering information as applied to industry in this country.

[fol. 1317a] SEYMOUR C. ANDERSON

I am Manager of Construction for the Sales Department of the Standard Oil Company of Kentucky with whom I have been associated since November, 1917, before which time I worked for the Standard Oil Company of Indiana, having been at the Wood River Refinery from July, 1914 to November 1st, 1917. When at Wood River, I was in the Engineering Department, principally employed on designs for the commercial use of the Clark stills. In the early

part of 1915, we made a complete set of drawings of the Clark tube still for use at Whiting. The Clark still was largely developed by E. M. Clark, now a Director of the Standard Oil of New Jersey, at Wood River and while I was there. The set of drawings was made for the Whiting refinery to follow in building stills similar to the Clark stills at Wood River. These stills were practically entirely developed in practice and put on paper afterwards.

The drawing appearing in the file wrapper of the Burton and Clark patent, No. 1,448,254, was made by me. It is dated May 5, 1915, which is the date on which it was completed. I have a distinct recollection of the funnel-shaped device marked in the drawing "Galvanized iron hopper top, 40" x 24"." The apparatus of this drawing had been commercially and successfully used on quite a few stills in the first battery of tube stills before the draw-
[fol. 1317b] ing was made. I personally remember going into the plant and shop and getting the details of the drawing from the actual construction.

The file wrapper and contents of the Clark patent was offered as Defendant's Exhibit 406 and admitted in evidence.

Said Exhibit is omitted from the record because its only pertinent part is the dated drawing, herein identified, which illustrates with substantial accuracy the drawings of the said Clark patent.

[fol. 1317c] CHARLES W. SHAEFFER

I have been Manager of the Providence Refinery Company of the Standard Oil Company of New York during the entire six years that Refinery has been operating. Only recently, Burton pressure stills of the tube type were in use. I was at the Refinery and familiar with its operation when it was visited by the American Institute of Chemical Engineers in June, 1925 and was familiar with the stock used in the Burton process pressure stills.

I want to call your attention to some questions and answers that were asked and given on the examination of one of the witnesses for plaintiff, Professor McKee.

The Master: Dr. McKee?
Mr. Dorr: Dr. McKee.

Mr. Rauber: What page was that on?

Mr. Dorr: That was, I believe, No. 580 of the printed record.

"Question. Do you want to mention the Chamberlain patent? I think you did mention that in your direct.

"Answer. I overlooked that. It was not in my book. I think, on page 8, lines 43 to 59, we have one of the two phases which are the essence of this patent:

"'In reciting in claims following that the oil is free from still wax and like heavy matters of sticky character, and from elements yielding the same, or coke, a practical rather than a theoretical perfect freedom is to be understood. It suffices, for example, if the oil operated upon be [fol. 1317d] distillable in an externally heated still of the dimensions hereinbefore mentioned for still, A, charged 6/10th full under a super-atmospheric pressure to seventy-five pounds to the square inch at the aforesaid rate of two per cent per hour to the extent of twenty per cent or more by volume of its component hydrocarbons which have viscosities above 70 seconds 100 degrees Fahrenheit without heating the still bottom to redness.'

"When it is borne in mind that, for example, the Burton process carried out at Providence distills considerably more than this from a raw oil and from most any raw oil, this requirement which, on its face, looks like that it is a limitation, means nothing; * * *"

And again, this question and this answer:

"Question. What was the stock that they were using in Providence for their charging stock?"

"Answer. A crude carrying about one-half of 1 per cent of sulphur. I was not particularly interested in the subject when I visited it but I inferred that it was a rather low sulphur Mexican oil."

Now, at the time, Mr. Shaffer, of this visit of the American Institute of Chemical Engineers at the Providence Refinery in June, 1925, was the refinery, in fact, using this charging stock in the Burton process stills, Mexican crude oil or any other kind of crude oil?

A. No.

Q. Has the refinery ever used such Mexican crude oil or any other kind of crude oil as charging stock for Burton pressure stills to your knowledge?

A. No.

Q. Has the refinery ever used any residuum of crude oil after taking off gasoline or kerosene or any other cuts as charging stock for these pressure stills?

A. No.

Q. How is the charging stock which has been used in pressure stills—in the Burton pressure stills at your Providence refinery been obtained?

A. About equal parts of paraffin distillate and gas oil.

Q. Does your refinery keep records from which you can trace the charging stock which has, in fact, been used?

A. It does.

Q. In the pressure stills——

The Master: At any particular time?

Q. At any particular time.

A. Yes, we keep those records and have them with us.

Q. Have you kept complete records ever since the refinery was opened?

A. Yes, sir.

Q. Have you brought down here the records for 1925?

A. Yes.

Mr. Dorr: That is what I proposed to do. I offer to submit these records for the year 1925.

[fol. 1317e] It will take an explanation of the way the books were kept, but Mr. Shaeffer is more familiar with that and will be very glad to give any explanation so as to show just how these records were kept and how that particular record can be traced.

I am entirely willing to tender not only those records but the records of the refinery du-ing the whole period that the Government desires to examine.

Q. Would you, as a practical refiner, attempt to run any crude oil or reduced crude through your Burton pressure stills?

A. No.

Q. At what temperature did you crack in the Burton pressure stills?

A. About 740,750 degrees Fahrenheit.

The Master: In using the so-called Burton pressure stills at your refinery at Providence, did you follow the method and process of the Burton patent?

The Witness: Yes.

Mr. Dorr: That is all.

Cross-examination.

Questions by Mr. Rauber:

Q. Mr. Shaeffer, do you know Dr. McKee?

A. No, sir, I do not.

Q. What kind of crudes do you take into that plant at Providence?

A. Take in North Texas crude and Mexican crude.

Q. Were you taking in Mexican crude at the time that Dr. McKee was there in June, 1925?

A. I could not answer your question without referring to the record, whether we had taken anything in at just about that date, but I know that we had Mexican crude in stock.

The Master: Have you examined to see whether, on the date in question, June 25, 1925, is that it?

The Witness: Right.

The Master: Whether the distillate that you were using for charging stock for your Burton stills was derived from Mexican or from American oil.

The Witness: We have examined—I have examined.

The Master: You have examined?

The Witness: Yes, sir.

The Master: From what crude oil was the distillate derived?

The Witness: North Texas crude.

[fol. 1318] Robert E. Wilson, a witness on behalf of the Defendants, being recalled, testified as follows:

Direct examination.

Questions by Mr. Cooper:

The Hall patent 1,175,909 and also the other Hall patents which I will consider, differ from the types of process

I was discussing in my prior testimony in being essentially tube-cracking processes.

In this patent Hall describes a process by which heavy hydrocarbons are subjected to the action of heat and pressure in a long directly fired tube, the oil stream being heated to approximately 600 degrees centigrade or 1,132 degrees Fahrenheit, under pressures of about five atmospheres.

Hall specifies the possibility of using nickle or other — in his reaction tubes, and that he does not use steam or any outside source of hydrogen. He also specifies that while his pressures are substantially above atmospheric, they are not enough to prevent vaporization of the liquid at the temperatures in question.

The particular feature in this patent, in addition to the catalysts, which are one feature not of direct interest to us, is the sudden expansion of the vapors at the valve shown in the drawing to the immediate left of the expansion chamber in the center of the drawing. By suddenly reducing the pressure at this point and discharging into a large vessel, where the velocity is slowed down. Hall claims that he minimizes the deposition of carbon in the tubes, and deposits relatively large amounts of carbon just beyond this point of expansion.

I may say that in my experience Hall is correct in that, that just beyond an expansion valve in this type of process there tends to be a localization of a carbon deposit, and the use of an expansion chamber of some sort very closely beyond the valve is important to prevent clogging up the apparatus.

The references cited against this by plaintiff's expert include Benton, Testelin and Rennard, Kittle and Schuchow and Gavrilow. Of these, Benton is quite far removed, be- [fol. 1319] cause he clearly discloses a kerosene-making process in which the lighter oils like benzine are converted into kerosene, and he also clearly specifies the use of pressures sufficient to prevent vaporization, which is quite different from the disclosure of Hall.

With regard to the other three, in none is there the step which I mentioned, of allowing the vapors immediately after expansion to pass into a large unfired carbon-collecting chamber, which does have real value, as I indicated.

The Master: Does that valve open and close periodically?

The Witness: I should judge no—this being a continuous process, like a tube cracking process, the valve was merely adjusted to maintain the desired pressure—in other words, acting as a throttling valve, always partly open.

Also, Testelin & Rennard have some steam in their process, which Hall specifically excludes.

The Master: It passes the valve, then, under the pressure in the tube?

The Witness: Before the valve it is under a pressure in the latter end of the tube.

The Master: After having passed the vavle, it is——

The Witness: It is substantially reduced.

The Master: It drops to the pressure in the expansion chamber.

The Witness: Yes, sir.

Q. Now, will you look at Hall's patent No. 1242795, which is petitioner's "Exhibit No. 183," and No. 1242796, which is petitioner's "Exhibit No. 184," and describe the salient features of those, and discuss the prior art cited by petitioner's expert, against them.

A. These two patents are similar in most of their features.

The first patent, No. 1242795, shows a tube cracking process, operating at 70 to 80 pounds, and at 600 to 650 degrees Centigrade, corresponding to 1112 to 1202, degrees Fahrenheit, in which the features emphasized are the partial cooling of the vapors to about 200 degrees Centigrade, or 392 degrees Fahrenheit, while still under pressure, these pressure coolers being B and B′, on the drawing on the separating chambers, C and C′.

He then takes the lighter vapors which remain uncondensed and compresses them to a higher pressure in a compressor shown diagrammatically in F, and causes them to commence at a substantially higher pressure than that pre- [fol. 1320] vailing in the cracking tubes—at least, at the outlet of the cracking tubes—and finally collects his materials.

The other patent, No. 1242796, is very similar, except that the preliminary condensation of the—or the intermediate condensation of the heavier parts of the distillate, is carried out at reduced pressure. The pressure is reduced, and then the pressure is again increased before the final condensation.

In both of these patents Hall presents a theory as to what happens in his final condensation, to the effect that chemical combination takes place between his lighter condensable products and the main part of the distillate—which, I believe, has not been substantiated, so far as I know, at least.

It is true, however, that by following the operation which Hall suggests, you do recover a much lighter and yet satisfactory stable product, that can be obtained without that step, or some equivalent step of re-compression of the vapors.

You will recall that in the bulk pressure distillation processes, I pointed out that it had been found quite desirable to separate the gases from the liquid, while the gases were still under pressure, in order to minimize the amount of vapor carried off; and, furthermore, that the optimum pressure for that separation we had found, experimentally, to be in the neighborhood of 75 to 80 pounds. While higher pressures attended the reduced amount of liquid going out with the dry gas, that tended to increase the amount of gases which dissolved, which neutralized them again.

In tube cracking processes, however, on account of the much larger proportion of gas formed, to the amount of liquid, the optimum pressure, for this condensation, is considerably higher—that is, you gain by going to pressures above a hundred pounds, more than you lose by the increased solubility of the gas and the liquid.

So, by this process of first condensing out, separately, the heavier ends, and, second, by re-compressing and recovering the light ends, Hall recovers a material fit, after certain refining processes, to be a very satisfactory motor fuel, and distinctly more volatile than that obtained by ordinary tube cracking processes, where this step of re-compression of the vapors is not carried out.

Hall's product is—I will discuss the product later.

Against these process patents plaintiff's expert apparently cited Dewar & Redwood, Nikiforoff and Lang, processes, which we have previously considered.

Of these Dewar & Redwood is, of course, a bulk pressure distillation process, carried out at low temperatures, and does not involve any step of re-compression, and does not make the lighter motor fuel which Hall particularly specified. So it is about as different as two processes could be.

With regard to Nikiforoff, he used no pressure whatever. He did not have any pressure condensation, and so, of course, didn't have this feature of Hall's, which I have mentioned.

As to Lang, who did, to be sure, condense under pressure, plaintiff's expert stated, in effect, that if Lang had operated at Hall's temperatures, and had condensed at the pressures which Hall recommended, he would have had the same product as Hall.

But the fact is, first, that there is no such disclosure in the Lang reference, whatever, and, secondly, that Lang particularly does disclose the product which he obtains. As I pointed out, it was liquefied gas, suitable for snow melting, which he was particularly interested in. So that that, also, is quite different.

By reason of the temperatures at which he operates, namely, between 1100 and 1300 degrees Fahrenheit, the product which Hall obtains is quite different in characteristics from the product of the low temperature, bulk pressure distillation processes which we have considered, being higher in olefine content, with a certain amount of aromatic compounds, heavier in gravity and a larger ratio of carbon to hydrogen than those products.

As compared with the products of tube-cracking processes, of course, the very high temperature products and the low temperature tube-cracking products, both were somewhat different in character from this intermediate material in these same respects. But, in addition to that, his process of first condensing out all the material boiling above 200 and then re-compressing so as to recover the light ends which are ordinarily carried away with the large amounts of gas made in these processes, gives him quite different volatility char- [fol. 1321] acteristics and much more satisfactory for motor fuel than they would otherwise be.

These properties of his products are described in considerable detail and he is, I believe, the first to point out a product which had very marked anti-knock value. He was really several years ahead of his time in this application filed in 1915, in pointing out that this particular product was markedly anti-knock in its characteristics, and had other advantages in greater mileage. His statements are also rather unique in claiming for a new product only minor improve-

ments in mileage, instead of the customary extravagant claims of improvements in mileage and in power, which can actually be verified and have on similar products been verified by myself.

Comparing the two patents, the second patent refers to the refined product, while the first refers mainly to the crude or unrefined product.

Q. There is one other patent I would like to call your attention and that is that of DeFlores, Patent No. 1,437,045, issued November 28, 1922, and, like the Hall patents that you have discussed, the property of the defendant, the Texas Company.

Will you compare its disclosures with that of the Hall process patents which you have explained?

A. The general type of process is quite similar to that of the Hall tube-cracking patents, which I have just discussed; and also similar in taking off an intermediate heavy cut before getting the desired product. De Florez, however, instead of re-compressing his vapors immediately after the heavy part of the distillate has been condensed and removed, continues to condense at the lower intermediate pressure and condenses as much as he can under those conditions and then re-compresses only his tail gases. This gives him a smaller volume to re-compress and at a lower temperature, which means that he must use some substantial means for recovering the rather wild products which are thus produced and getting them back into the main body of the distillate and mixing them therewith.

He suggests for example, the possibility of scrubbing out these products with oil from the compressor. That is the principal element of novelty in the DeFlorez patent, as compared with the Hall patent.

I think Hall was the first to disclose a fuel having definite anti-knock properties.

I stated, in response to a question of Mr. Wiles, that the more recent construction has very much predominated in the construction of tube stills, and I stated in response to a question on the next page, that I was considering particularly construction since 1918 or 1919.

I was basing that on my knowledge of the fact that our last sixty stills at Whiting have all been tube stills, and that the stills, something like 150, installed at Bayway within

that period were tube stills, and that the Standard's of Louisiana installation of 80 or 100 stills were tube stills.

I find in looking at the records, however, that during that same period our Western refineries have constructed a fairly large number of shell stills; so the predominance is not quite as great as I considered, although the trend is certainly in that direction.

[fol. 1322] ROBERT E. WILSON, recalled by the defendants, testified further as follows:

Direct examination.

By Mr. Wiles:

Q. Mr. Wilson, in the course of your testimony you discussed the graphic formulae for a number of hydrocarbon oils, and I have asked you to prepare a single sheet showing the graphic formulae of typical hydrocarbons. Have you done that?

A. I have.

It shows what I consider to be the most probable structural formulae for one member of each of the four classes of hydrocarbons—paraffin, naphthene, olefine, and aromatic—together with their structural formulae and certain statements regarding the arrangement of the chains. The illustration is in each case for a member of the family with seven carbon atoms. I also indicate why the asphaltic unsaturated hydrocarbons are not presented, because we do not believe there is anything that has as few as seven carbon atoms, and we do not know just what their structure is.

Q. You have a generic name and a class name over each of those. What do those mean?

A. Well, just the broad class and the narrow class. I think they are clear from their terms.

The Master: This will compare with some of the tables produced by Dr. McKee?

The Witness: Yes, and practically all data which I myself gave, but scattered throughout the testimony so I only mean it for reference.

Mr. Wiles: I will offer that in evidence as Exhibit No. 606.

(Whereupon said Exhibit so offered was marked Exhibit 606, received in evidence, and the same is returned herewith.)

Cross-examination.

By Mr. Rauber:

Q. Mr. Wilson, are there any other ways of representing that hydrocarbon toluene?

A. Yes, as I indicated that the Kekule's formulae is sometimes used with three double bonds in the ring; but I do not consider it the most probable formula by any means.

Q. That is the most commonly accepted at the present time, isn't it?

A. Why, if you ask a chemist just to draw something in a hurry, I think the majority would draw that; but if you asked one if that really represented its composition, he would certainly hedge and say no, that is just a convenient method of representing it.

Q. He would hedge on this, too, would he not, that you have put in—the Bayer formula?

A. He would do, I think, just as I have done, and say—I have gone into this more in detail in my direct testimony—that there is an extra bond per carbon atom lying around loose somewhere. All we know about it is that it does not behave like an ordinary double bond in straight chain, and in some way we believe the extra bonds are all tied up inside the ring and tend to give some of the remarkable stability which this possesses.

Q. But you can add on chlorine and other elements, can you not?

[fol. 1323] A. With great difficulty.

Q. Difficult to add clorine on to benzene and toluene?

A. Much more difficult than to add it on to an ordinary double bond. Of course, it can be done. We know methods of doing it. Another indication of the same thing is that once you add one pair of hydrogens on, immediately the whole structure seems to change and it goes to a form which probably is represented by the double bonds. You cannot just add hydrogen to one double bond and have the other two left. If it goes at all, it goes the whole way.

Q. Then you can add on six more hydrogens, can you not?

A. Yes.

Q. Then you have the same thing as your saturated naphthenes?

A. Yes.

Q. In that respect it has the same relation to your naphthenes that your unsaturated olefine has to your paraffin?

A. In that respect that is true.

RALPH H. MCKEE, a witness on behalf of the Petitioner, being recalled as a witness for the Petitioner, in Rebuttal, testified as follows:

Direct examination.

Questions by Mr. Rauber:

Q. On page 1980 of the Printed Record, defendant's expert stated:

"Of the distillable portions of crude oil very little, seldom over 5 per cent of the constituents, are unsaturated or olefinic hydrocarbons."

Is this statement correct?

A. No. The distillable portions of crude oil often run as much as 50 per cent, and as confirming my view in this, I want to quote the analysis of a number of oils.

The Master: Now, this goes to the distillable portion of the crude—is that right?

The Witness: That is correct. That practically means the portion other than asphalt, because asphalt is measured as the part which is left after distillation.

The Master: Well, one moment, there, Doctor.

The Witness: I am referring to the analysis.

The Master: How about the other portions, which become solid?

The Witness: In the determination of asphalt the method is to distill off simply—that is, the gasoline and kerosene, and then run steam in and continuing to distill [fol. 1324] until there is a portion left which does not fur-

ther distill, and that is called asphalt. If it were run further by direct heat the asphalt would decompose and we would get coke and gas.

Q. That is, in the distilling method?

A. Yes, the ordinary government method.

Q. Not in the crude still?

A. No.

I am quoting from Volume 1, American Petroleum Industry, by Baker and Hamor, page 139.

A Kansas petroleum, carrying 41.2 per cent of unsaturated hydrocarbons, and asphalt of .39 per cent—that is, less than one-half of one per cent of asphalt.

On page 141, another one is given as containing 50 per cent of unsaturated hydrocarbons in a crude oil. The percentage of asphalt——

The Master: Is that Kansas?

The Witness: In Kansas, yes, sir.

These are mid-continent oils, and later I will give some other oils. The percentage of asphalt is quite low, and, in fact, is not given.

On page 158, from Oklahoma, we have another mid-continent oil, carrying 57.2 per cent of unsaturated hydrocarbons and .31 per cent of asphalt.

I may add that a very large proportion of those from Oklahoma run 20 to 30 per cent of unsaturated hydrocarbons.

On page 167—A Texas petroleum, carrying 59.6 per cent unsaturated hydrocarbons, and the amount of asphalt so low that it is marked as zero per cent.

Another, on the same page, runs 46 per cent unsaturated, and .40 per cent asphalt.

The Master: When you change from mid-continent oils——

The Witness: Texas oils are, many of them, counted as mid-continent oils.

The Master: I know—I wish, when you do change from mid-continent oils, you will designate it so that I will understand what you are talking about.

The Witness: I will do so. Page 168, the Texas oil is given as having 66.8 per cent of unsaturated hydrocarbons, and .74 per cent of asphalt.

Another one, on the same page, as 46 per cent unsaturated hydrocarbons, and 40 per cent of asphalt.

I am not quite certain, but at least the last of those that I have given you, from Texas, is a Gulf Coast oil, which is below the portion of Texas that is commonly included in the [fol. 1325] mid-continent field. Possibly some of the others of these Texas oils are mid-continent oils, but I don't know the county names in Texas.

Going, now, to another field—West Virginia—on page 181, a West Virginia oil is given as having 32.8 per cent unsaturated hydrocarbons, and the per cent of asphalt is not given, but is, presumably very low, as is the asphalt in most of that field.

On page 185, Wyoming, a western oil, is given as 32.4 per cent unsaturated hydrocarbons.

On page 188 we have nine out of ten oils from Wyoming, whose unsaturated content is given, all of which run at least 46 per cent unsaturated hydrocarbons, and several as high as 58 per cent, the asphalt running from zero per cent up to 15 per cent.

The Master: What are the intermediates between those percentages?

The Witness: In asphalt?

The Master: No, no—what are the intermediates——

The Witness: The intermediate oils?

The Master: Are they intermediate oils, or lubricants?

The Witness: They give them simply as the part that is distilled, for each group. Say, for gasoline—the amount distilling below 150 C., which would be a rather high test gasoline, for example—they vary from 1 to 14 per cent, and the amount distilling, f-om 150 to 300 C., which would be a rather——

Mr. Wiles: You mean Centigrade?

The Witness: Centigrade, which would be a rather low boiling kerosene—from 21 to 41 per cent. That is, they are typical oils, as we would take them, of the Western section.

The Master: Let me get the issue that you are testifying about, here, clearly in my mind.

Mr. Wilson says that there are very large percentages of crude not distillable. He says that a limited portion of it is distillable, and you say no, that a very large portion of it is distillable.

The Witness: No. The question was as to what per cent of crude oil is unsaturated.

The Master: Oh, I thought you used the word, "distillable."

Mr. Rauber: Of the distillable portion.

Mr. Wiles: It is conceded that asphalt is unsaturated, but it is not distilled.

The Master: Yes, of course.

[fol. 1326] The Witness: The point being that Mr. Wilson has said it was five per cent, and here it runs up many times, in the range of 40 and even 58 per cent.

I think those are probably sufficient to prove the point.

Mr. Wiles: May I see those, please?

The Witness: Yes.

Q. On page 1,983 of the printed transcript of record, Defendants' expert in speaking of shale oil stated: "From 70 to 90 per cent of these oil are soluble in concentrated sulphuric acid."

What have you to say in regard to the correctness of this statement?

A. I know of shale oils of which this is true. In fact, I made once a freak oil in which a hundred per cent of it was soluble, but that was on a laboratory experiment in which never more than three ounces were made. The commercial shale oils, however, which have been made are quite different.

As confirming this statement, I wish to refer to Bulletin 210 of the Bureau of Mines of Washington, in which analyses of shale oil from Scotch shale are given on pages 172 and 173, in which portions could be distilled without cracking at atmospheric pressure—that is, not having to use a vacuum—where the unsaturation is given in one case as 32 per cent and in the other, for another sample, as 31.9 per cent. It is rather well known that in a product of this kind the percentage of unsaturation does not vary greatly between the different fractions. In fact, the variation that there is *is* in the direction of slightly more in the light portions rather than in the heavy portions. The variation, though, is almost insignificant. These were for shale oil which had not been through any refining process.

Of course, shale oil will vary, even when made from the same shale, according to its method of retorting, and we

might expect that different lots might vary a per cent. That is, it will not run as standard as oil continuously from the same well or from the same pool, because it has been through a cracking process.

From the American shales we have products some of which will give a higher percentage of unsaturated than these which have been on the market hitherto commercially —say in the range of around 60 or 70, or somewhat above that figure.

Q. Further on page 1,983 of the printed transcript of record Defendants' expert stated that, "from 70 to 90 per cent of shale oils are made of compounds which are [fol. 1327] almost entirely absent from petroleum, namely, the olefines and the nitrogen compounds, where at least 95 per cent of petroleum is made of constituents, asphalt and paraffin and naphthene hydro-carbons, which are present only to a minor extent in ordinary shale oil. There is a certain amount of paraffines, of course, in the shale oil." What have you to say in regard to the correctness of this statement?

A. The statement that 95 per cent of petroleum is made up of constituents, that is, asphalt, paraffin and naphthene hydro-carbons, which are present only to a minor extent in shale oil, I have indicated in my prima facie case is not quite correct. The statement is made that 70 to 90 per cent of shale oils are made of compounds which are mostly entirely absent from petroleum—that is, the olefine and nitrogen compounds. The Scotch shale oil, which is the shale oil best known, and the French shale oil, which has been known as long but not as well, which are the two commercial shale oils which have appeared in the number of the citations, carry, as I have indicated, perhaps 35 and 40 per cent of olefines. At least, that has been reported in a number of cases.

I have already indicated that there are large numbers of petroleums which carry 40 and 50, and even 58 per cent of unsaturated hydro-carbons, in the quotations which I have given from Bacon & Hamor, and which are really analyses made by Government agencies and quoted by Bacon & Hamor.

Nitrogen compounds are present in many shale oils and also in many petroleums. The percentage of nitrogen compounds present in the French petroleum is, for exam-

ple, much less than that present in a California petroleum, being in the order of half as much. A California petroleum will carry from 10 to 12 and sometimes 15 per cent of nitrogen compounds; the other petroleums will carry smaller amounts. Pennsylvania petroleum will carry almost none. But so also this is true of shale oils, some of them carry practically none. So that we have the fact that the variation in nitrogen compounds is as great or greater in petroleums as in shale oil, and the amount of olefines carried by well known American petroleums—and probably the same is true also of petroleums from the rest of the world—is large, and as large as those shale oils which have been cited here in the citations.

Petroleum also carries asphalt, but so does shale oil. The Bulletin 210 which I mentioned does not give for the Scotch oil its percentage of asphalt. It does, however, give for American shale oils, the percentages of asphaltic materials in analyses run on some shale oils made from American shales.

[fol. 1328] For example, page 171 gives an asphaltic residue of 22 per cent from the shale oil.

The Master: American shale oil?

The Witness: American shale oil. An Indiana shale.

A. (continuing): Page 168, from Utah shale it does not give its percentage, but says the residuum is asphaltic. On a Colorado shale, page 165, it is 6.1 per cent asphaltic residue. On page 164, another one has 11.6 per cent asphaltic residue. On pages 163, 164 and 165, for Colorado shales, there is given an asphaltic residue of a percentage varying from 8 to 14 per cent.

The sulphur compounds might also have been mentioned there, but they also are in the same general range and the same general percentages as those present in the other kind of oil. You can not, roughly, tell the difference the shale oil and a well petroleum, so far as percentages of sulphur content. In fact, there is no way at present in which a skilled chemist, given a sample of oil, could say definitely whether it was a shale oil or a well petroleum. Except that as yet we have no shale oil, so far as I know, that is like the East Boreno shale oil, which is very largely aromatics; nor one which is the other extreme, like the Pennsylvania oils, which are also free from olefines.

The Master: You said, "Borneo shale oil." Did you mean that?

The Witness: No, I meant Borneo well petroleum. But of the intermediate group we could not say definitely from examining the oil whether it was a well petroleum or a shale oil.

The Master: I would like to be put straight on this. The significance of the distinction between shale oil and well petroleum lies in the material employed in the foreign patents cited, does it not?

The Witness: Yes.

Mr. Rauber: Not in all of them.

The Witness: Not in all of them.

The Master: No, but I mean in some of them. What I am getting to is this: The distinction, then, is between the material presumably worked on in those foreign patents—which was presumably Scotch or French shale oil, was it not; not American shale oil?

The Witness: Not American shale oil.

The Master: Therefore, American shale oil has little to do with the matter.

[fol. 1329] The Witness: Very little.

The Master: That is what I wanted to get in my mind.

The Witness: The viewpoint which I am presenting here is that the two are identical.

The Master: Oh, your point is, then, that American shale oil——

The Witness: No, that well petroleum and French shale oil are identical, and we might even include American shale oil.

Q. On page 2029 of the printed record Defendants' expert stated: "In bulk pressure distillation, the pressure, and particularly the corrolation of pressure with temperature, are of absolutely fundamental importance in determining just what is cracked and the correct products produced."

What do you say in regard to the corrolation of temperature and pressure disclosed in Burton Patent No. 1049667?

A. This patent discloses a pressure of four to five atmospheres, used with a temperature of 650 to 850 degrees.

The Master: What patent is this?
The Witness: The original Burton patent.
Mr. Rauber. Patent No. 1049667.

A. (Continuing:) This patent discloses, as I said, the use of four to five atmospheres with 650 to 850 degrees Fahrenheit temperature. The present process, as described by the Defendants' expert, is a process working at 90 pounds, which would be more than that given by the patent. That is, it would be six atmospheres instead of four to five atmospheres. Moreover, if we operated at 850 degrees temperature and used only a pressure of four to five atmospheres, or even six atmospheres, the product would be very largely kerosene, not gasoline, because we would be distilling over here and we would not have the proper correlation between temperature and pressure. The limit of temperature given here for the making of gasoline, which is the title of the patent, is far greater than corresponds to the limit of pressure. In other words, 850 degrees temperature operation would require, if making gas- [fol. 1330] oline, a very decidedly higher pressure, a pressure which would probably be several times four to five atmospheres—that is, more than ten atmospheres.

Moreover, it is to be borne in mind that in the original application there is no statement of pressure. This is shown in the File Wrapper, which is another exhibit.

The Master: Your point, then, is that the Burton patent, No. 1,049,667, by the very terms of its claim, is inoperative to produce gasoline?

Mr. Rauber: Oh, no, your Honor—the correlation is not stated. The temperature ranges away above what is required by the pressures given—as I understand it.

The Witness: That is correct.

The Master: That would have been the result if what the Doctor is saying on the thing, that the temperatures and pressures are not correlated to produce gasoline, but would produce kerosene?

The Witness: It would produce gasoline, if you would use an intermediate temperature of 720, we will say—or, in that range—but not operate well at 850.

The Master: Oh, well, that is different.

Mr. Wiles: In other words, if you used the temperatures set forth in the specification, it works fine.

The Master: It is simply this, that Burton saved himself by his range of temperature, bringing the temperature down to 650, and up to 850, and his atmospheres from 4 to 5.

The Witness: But he lacked the correlation.

The Master: Definite correlation, you mean? He gives you a range to correlate your pressures and temperatures in between?

The Witness: Yes, in part. It is effective for the lower half of his temperatures but not for the upper half. In other words, if you use——

The Master: I understand that you think that in the practice of the patent, you must have a 4 atmosphere pressure with 650 degrees temperature, and that nothing is left to the skill of the operator in that?

The Witness: No, no, I don't mean that, at all. What I mean is that this correlation of temperature and pressure, which defendant's expert said was absolutely fundamental in determining what is cracked, is not disclosed in Burton. He only partially discloses it, in that if you will take a 4 to 5 atmosphere, and his lower temperature, it will probably operate.

[fol. 1331] The Master: What you are saying, then, Doctor, is this: That the patent does not disclose, in that full and ample manner sufficient to inform one skilled in the art to practice the invention, the temperatures and correlation of temperatures and pressures? That is practically what you are saying, isn't it? I am only concerned in getting what it is that you are saying—that is all.

The Witness: If an operator took Burton's patent and used his high temperature it would be inoperative. If he used lower temperatures, of those given, it would be operative. But it lacks that correlation which has been given as absolutely of fundamental importance.

On page 2039, defendant's expert stated that it was the common impression, prior to 1913, that cracking reaction was absolutely due to the sudden heating of those drops of oils which were condensed on the top shelves of the still, and dropped back into the highly heated residuum. Is that statement true? At that time he was speaking of an atmospheric still, by the way.

Mr. Wiles: That is objected to, your Honor, as an improper resumé of the testimony. The witness, as I recall it,

gave three or four theories in a row, and said that none of them were right. That is not the only theory he gave, by a long ways.

The Master: Well, that is a matter of argument, when you get to it. Of course you may have misapprehended what the witness did say, and if you are, you are shooting at a blank. Answer the question.

A. This is not correct. For example, Aisinman, in 1905, in "Exhibit No. 118," page 1483, states:

"In order to return heavy vapors that have been carried off, and submit those to renewed association, the boilers are furnished with dephlegmators."

This would, at once, show that it was well known that it was not due to the sudden heating of those drops, but that we here have a returning of a stream from a dephelgmator back to the still.

Q. On page 2044, defendants' expert states that Benton, in patent No. 345,564, states clearly that at several points in this specification, by the use of his process, benzine, present in crude oil to an extent as high as 12 per cent, is converted into homogeneous high flash illuminating oil. Benton's disclosure, therefore, certainly did not lead anyone to try to crack kerosene to make gasoline, since he indicates that the reaction occurs in the reverse direction.

Is this a fair, or reasonable interpretation of Benton's process?

A. This is not a fair statement or interpretation of it, and it is well illustrated by the fact that Redwood, in "Exhibit No. 107," page 10386, in discussing this patent, states:

"The process consisted in pumping the refuse of petroleum oils, tar oils, and other heavy mineral oils—to produce lighter rydrocarbons from the products specified."

This is the obvious interpretation of the Benton patent, rather than that which has been presented by the defense, and is illustrated here by one of the authorities of the petroleum industry—perhaps the highest authority in the English speaking world—in the interpretation of this patent.

Q. On page 2081, defendant's expert contends that Krey's gas oil is entirely different from the gas oil he con-

tends was disclosed as a charging stock, by Burton. Is this contention true, or accurate?

A. Krey, at the Riebeck plant, was using gas oil from several types of well petroleum, as well as gas oil from lignite, and I will later show that gas oil from lignite is essentially the same, also, as that from well petroleum. He particularly mentions that from the Baku, or Russian petroleum, and he also runs the re-run oil, so that these processes carried out by Krey are carried out not only under approximately the same pressure as Burton, but also on the same materials. But with this difference, in that he condensed at atmospheric pressure, instead of condensing under still pressure. The materials, then, are essentially the same, and differ no more than gas oils from American petroleums would differ.

[fol. 1332] Q. On page 2062, the defendant's expert stated that the bulk distillation processes of the prior art all disclosed very dirty stocks. Is this statement true?

A. No. For example, the operations by Krey, which I have just mentioned—the operations by De War and Redwood, which use intermediate oils—and intermediate oil is simply an English term for gas oil—or includes gas oil—also—well, those are sufficient.

Q. On page 2063, defendant's expert stated that none of the prior art cracking processes operated on a commercial scale. Is this statement true? That is on page 5078 of the Typewritten Transcript.

A. This statement is not true. Young's process was operated, and he received royalties for many years, during the use of his cracking process.

Q. Were there any other commercial processes?

A. The Riebeck process was—I want the exhibit on page 40238—Krey carried out his processes in the Riebeck plant, on a commercial scale, and it is so described in the exhibit which we have here.

Q. What exhibit?

A. "Exhibit No. 247,"—I thought there was another one—but, "Exhibit No. 247," describes the commercial operation in the Riebeck plant.

Q. On pages 2063 and 2064, defendant's expert stated:

"The third, and in many ways the most important reason why the prior art processes used dirty stock, is that they were mainly doing their light cracking to make kerosene,

and that is a very much simpler and very much easier problem, from the standpoint of coke formation, than heavy cracking that is required to make gasoline."

What do you say in regard to this statement?

A. Riebeck used gas oil and he cracked to get approximately the same yield of gasoline as Burton got.

The Master: You say "Riebeck"?—do you mean Krey?

The Witness: I mean Krey at the Riebeck plant. The percentage being, as I recall, a distillate, in one case, of 58 per cent., and in the other, 60 per cent.

Riebeck also did cracking, using dirtier stocks, as is sometimes done at the present day, in American cracking plants.

In this connection I think I ought to mention that cracking to produce kerosene always produces gasoline, and often in approximately equal amounts. And so it is approximately as heavy cracking, excepting for the cracking of the kerosene itself. In other words, it is approximately as heavy a cracking of the other portions—say gas oil—as in the cracking when gasoline, only is wanted.

Q. On page 2077, defendant's expert further states, in regard to the prior art, that there are no such disclosures of available stocks which were clean enough to accomplish Burton's result. Is this statement true?

A. No, because the available stocks, as described, such as Dewar and Redwood's intermediate oils, and Riebeck's gas oils, are identical with the stocks which Burton would use, and, accordingly, are just as clean stocks.

Q. Now about the stocks used in the patent, or disclosed in the patent to Kittle? I haven't the exhibit number. What is that?

A. In "Exhibit No. 131," Kittle specifically mentions gas oil as his cracking stock, and consequently this would be just as clean a stock as suggested by the defendant's expert, for Burton—in fact, it is identical stock.

Q. At page 2084, defendant's expert states that Dewar and Redwood disclose a cracking of a reduced crude oil. Is this statement true?

A. Dewar and Redwood suggest the use of a natural petroleum, or similar oil, made from shale, but the preferred material to be used there, is an intermediate oil. So that they do disclose the use of a crude oil, but not a reduced

crude oil. They preferred an intermediate oil, which would be the same as a gas oil.

[fol. 1333] The Master: Is that his expression—"I prefer an intermediate oil?".

The Witness: Yes, sir, and this is better illustrated in Redwood's detailed description of the patent.

Q. Can you give the page on which that description occurs, that you now refer to?

A. No, I can not, just at the moment. I can find it. It is "Exhibit Nos. 107 and 103."

Q. Which one are you referring to?

A. I am referring to both.

Q. You find it in "Exhibit No. 107," do you not?

A. Yes, sir, on page 1389.

"With such apparatus intermediate and heavy oils may readily be converted to a considerable extent, into higher oils."

Mr. Wiles: Is that the quotation that you got the "preferred oil," from?

The Witness: That is one of the places. I think it occurs also, in another place or two.

Q. On page 2094, defendant's expert stated that in the operation of pressure stills condensing under pressure, that at times there would not be enough gas unless you had a lot of stills connected together.

Did Dewar and Redwood, in "Exhibit No. 107," disclose the use of several stills connected together in such a manner as to accomplish this result?

A. They did. Page 1389 is the description.

Q. What do they say about it?

A. "Although only one retort and condenser are shown, there may be several sets of these in communication with the oil and gas pumps, or with each other, suitable cocks, or valves being provided for the communicating pipes, so that several retorts may be worked simultaneously, or in rotation."

Q. Would it be possible to so connect them without connecting the vapor phases, and have it operate successfully?

Mr. Wiles: That is objected to as immaterial. The question is whether they disclose the doing of it. The possibility is not material.

The Master: It is a question of mechanics. It would be only his opinion about its possibility, or about its practicability, or about its being suggested or disclosed by the patent.

Mr. Wiles: The material question, to my mind, is whether it is disclosed, not whether it is possible.

[fol. 1334] The Master: It is only his opinion as to whether it is, or is not.

Mr. Wiles: I know. Do you find any such disclosure?

Mr. Rauber: I think it is more than that. If he connects two stills together, he must connect them by a pipe, and if he has a pipe connecting the liquid phases he must have them connecting the vapor phases, also.

The Master: That is an argument.

The Witness: He does disclose it, inasmuch as he makes his pipes plural. If you are going to connect two stills and condensers and retorts together, by "pipes," you would have part of it for liquid and part of it for the vapor phases, as anyone skilled in the art would at once do.

Q. On page 2131, defendant's expert, in answer to a question by the Master, stated that there was no citation in the prior art, of any tube still for distilling petroleum, or petroleum charging stock, under pressure. Is that statement correct?

A. There were three citations which, as I recall, gave methods of using a tube still in distilling petroleum charging stocks under pressure—Kittle, Pielsticker and Schuchow and Gavrilow—all are pressure methods, using tube stills—or, rather, tube and tank stills—which was what was referred to here. They are references which have previously been cited.

Q. In distilling tar, as in the De Clerque Patent, Exhibit No. 130, is carbon formed during the distillation?

A. Yes.

Q. Where would this carbon be deposited in the De Clerque operation?

A. Largely in the unheated tank and not in the tubes.

Q. On page 2150, Defendant's expert, speaking of the Dewar & Redwood process in reference to the run-back stated that "the means which they showed for accomplish-

ing that would neither give the close fractionation of the distillate which I have shown is very important to Humphreys, the heating and stripping actions to remove the gasoline or the very light ends from the return stream, and, furthermore, he was not dealing with the same distillate at all as Humphreys was concerned with." What have you to say in regard to this statement?

A. This statement is apparently based upon a supposed advantage of the straight pipe over the bulb form of still-head or run-back, such as Dewar & Redwood have. It has been rather well known for a number of years in chemical [fol. 1335] technology that a straight pipe is the most inefficient form of still-head.

I wish to read, in confirmation of the above, from Young's "Distillation Principles and Processes," page 127. Young is the man who has studied distillation probably more from the scientific side and is a world authority on this subject. He writes:

"Of all possible forms, the plain vertical still-head is the least efficient. During the distillation as the vapor rises up the cylindrical tube, the outer parts of it come in contact with the condensed liquid flowing down the sides of the tube. Since the tube is constantly losing heat by radiation, and by conduction to the surrounding air, this liquid is slightly cooled and fresh condensation constantly goes on, the outermost layer of vapor probably condensing almost completely without much change of composition."

The Master: About when was that written?

The Witness: This is a late edition, 1922. But we were discussing——

The Master: No, I mean when did Young write it?

The Witness: This is a 1921 preface, so that he wrote it just before that. I think it is also present, in fact, I know that somewhat similar statements are present in the early edition of 1903, but I was not able to get it, as it is out of print at present. On page 131:

"It has been pointed out that the inefficiency of the plain vertical still-head is due to the want of through contact between the vapor and the condensed liquid, owing, firstly, to the central portion of the vapor passing rapidly up the

tube and possibly never meeting with the liquid at all, and, secondly, to the condensed liquid flowing very rapidly back to the still. Consequently, any modification that brings about better admixture of the ascending vapor or that retards the downflow of condensed liquid should increase its efficiency."

Another reference which indicates again the advantages of the Dewar & Redwood type of still-head over that of Humphreys, is shown on page 17 of Gatterman's volumes on the Practice of Organic Chemistry, Eleventh German Edition, of 1912, in which he illustrates the Wurtz type of still-head in Figure 15.

Q. Does he call it a still-head there, Doctor, or a reflux condenser?

A. He speaks of it as a fractionation apparatus. Perhaps I should read this in translation.

[fol. 1336] Q. I do not think that will be necessary, if you will just tell us what it is.

A. In comparing the use of a straight pipe he says that a quicker and a much more complete fractionation can be obtained if we use the fractionation apparatus illustrated in Figure 15, which is——

The Master: What is the fractionation apparatus in Figure 15?

The Witness: As apparatus with two bulbs on it, which would give the equivalent effect, though on a smaller scale, of Dewar & Redwood. In fact, it is essentially the same type of apparatus.

Q. On the same page, 2150, in reply to a question by counsel, Defendant's expert states that "if you use additional cooling means in the Dewar & Redwood reflux a part of the vapors would be overcooled and the rest of them hardly cooled at all." Is this statement correct?

A. The advantage of the Dewar & Redwood shape of reflux is just to prevent this cooling condensing of the vapors as a whole, as compared with the condensation in part—that is, to get a fractionation effect. In other words, it is the kind of apparatus we would choose to do this, rather than take a straight pipe such as Humphreys and such as Young have indicated as being the one which produces just

this effect, that is, of condensation of all the vapors on the outside, leaving core of unchanged vapors go through.

Q. You mean a straight pipe would?

A. A straight pipe would. With the change of diameter of pipe, coming out and going in again, we have a churning up of the vapors, accompanied by a change of velocities, and accordingly we would have a better fractionation effect for the same volume and same service.

The Master: In what sense do you use the word "fractionation"?

The Witness: In the sense of condensing one material and running that back—such as we are concerned with here, the condensing of kerosene and gas oil, and allowing the gasoline and some kerosene to pass over.

The Master: Then you are not using it in the sense of the step of final recovery and separation of the different products?

The Witness: A partial separation of the products, but not complete. The expectation would be, I should think, that even with Dewar and Redwood we would redistill our product.

[fol. 1337] The Master: After it has been fractionated and run back?

The Witness: No, I mean that the product which would be condensed in the condenser would then be chemically treated or redistilled.

The Master: Yes, but that is far away from this entirely. That is the sense in which I thought you were using the word "Fractionation." In other words, your cracking re-action is practically done, and it then reaches the stage of separation of the lighter and heavier products, then carrying them to condensation and recovery. That is the sense in which you use "fractionation," is it not?

The Witness: I am not certain that I understand this question right.

The Master: The distinction is this? Humphreys is not that final step at all.

The Witness: Nor is Dewar & Redwood.

The Master: It is an intermediate step. If you are talking about fractionation in the final step, your final recoveries, that is one thing; if you are talking about fractionation in the intermediate step, that is another thing. As I un-

derstand it, Humphreys inclined to an intermediate step in his process. Are you keeping that distinction in mind?

The Witness: Yes, I agree thoroughly with you. But the criticism, as I recall, of the defense, was that the Dewar & Redwood run-back would not be an efficient type. My point that it is, as a type, a more efficient type than the Humphreys straight tube effect.

Q. Efficient for what purpose?

A. Efficient for permitting the desired product to go on through and running back—that is, fractional condensation of the part which is wished to be re-cracked.

Q. And better suited for stripping?

A. Yes.

The Master: Just so long as I am getting clear in my mind what you are talking about, that is sufficient. The rest of it is all for argument.

Q. On page 2166 Defendant's expert states in the Barbet Patent, Exhibit No. 136, "we have the only example I have been able to find of any kind of cracked vapor going through a bubble-tower." Are there other examples not in the prior art, of passing cracked pressure still vapors through a bubble tower?

A. Yes, we have the use of Pielsticker's bubble tower for the passing of cracked vapor through it, and we have, of [fol. 1338] course, several other Barbet patents in which this is true, including the United States patent.

Q. You mean the United States patent to Barbet?

A. Yes.

Q. I think perhaps he had reference to that.

The Master: Is that the same disclosure, only in a United States patent?

Mr. Rauber: Practically the same. I intended to include it.

The Master: They are the same disclosure?

Mr. Wiles: I would like to get for my own notes what are these several others?

The Witness: Of Barbet?

Mr. Wiles: Yes.

The Master: There is really but one Barbet disclosure, isn't there, represented by the foreign patents and the American patents? It is all one disclosure?

Mr. Rauber: I think that will be sufficient for us.
Mr. Wiles: You disclaim these several others, then?
Mr. Rauber: Yes, except insofar as he mentioned them.

Q. On page 2167, Defendant's expert states with reference to the material used in the Barbet patent, Exhibit No. 136 or No. 141:

"It is crude petroleum, the word 'petroleum' being used presumably in the European sense for shale oil."

Do you find any justification for this assumption?

A. I do not find justification for this assumption, from two or three viewpoints. In the first place, France was importing at this date roughly 100,000,000 gallons of well petroleum from America, besides what she was getting elsewhere. Her own production of shale oil only amounted to 4,000,000 gallons a year.

The Master: What year was that?
The Witness: Around the period from 1900 to 1910, when Barbet was working on this subject. So that at this time there were twenty-five times as much American well petroleum taken into France as there was shale oil being made in France. Or, in other words, well petroleum was common material. I can give you detail data on——

Q. Where did you find this date, Doctor?
A. I got this data by going to the Customs House here and going over the exports of oil during this period and determining how much was shipped into France.

The Master: That is crude?
The Witness: Of crude and of finished products also. I think I have all of the data here.
[fol. 1339] Q. Was this crude oil or finished products?
A. The hundred million gallons I mentioned were crude oils.
Q. Not finished products?
A. Not finished products. The amount of finished products was very much greater.
Q. On page 2168 Defendant's expert stated he was not able to say that the function of Barbet's bubble tower was not the same as that of the Lewis & Cook patents but that

he should doubt very much that it was the same. What do you have to say on this point?

A. The Barbet bubble tower does all that the Lewis & Cook tower does, and it also does other things as well. It functions, in other words, essentially the same.

Q. And it operates on cracked products, does it?

A. It does.

Q. On pages 2171 and 2172, in speaking of the firing of the coil and of the drum in the Pielsticker's apparatus, Exhibits Nos. 133 and 134, Defendants' expert stated that he "would draw the same inference which plaintiff's expert drew, that they contemplated roughly equal amount of heat application or firing, or both." What have you to say as to this statement?

A. The furnaces underneath the coil and underneath the drum in the Pielsticker citation, are of essentially the same size, and might be expected to burn the same amount of coal. However, there would be much more heat absorbed in the coil than in the drum, from an equal amount of fuel applied, for several reasons. In the first place, the oil entering the coil is cold, or approximately at room temperature; it is not at its final temperature or anywhere near it.

Q. That is at the start, or the first part of the coil?

A. In the first part of the coil. It will get hotter as it goes through the coil. In the second place the coil offers very much more surface for a given volume of oil, and consequently would absorb heat much more rapidly and readily. Again, we have the fact that some cracking does take place in the coil, and more cracking in the tank or drum. The cracking in the coil, however, is only the beginning of the cracking, and would only take place in the latter part of the coil. This cracking of oil will absorb heat, so that we will have some lowering of temperature—or rather not increase of temperature—due to absorption of heat at the last pass or two of the coil, and we will have this to a considerable extent through the whole of the drum. In other words, this absorption of heat by cracking occurs to some extent in the coil, and to a very considerable extent in the drum. So that all told we will have a much greater amount of heat absorbed by the oil in passing through the coil, and a much greater rise in temperature than we will by the oil in the drum.

Q. Assuming equal firing in both cases?

A. Assuming equal firing in both cases.

Q. Later on, the same page, that is at page 2172, Defendants' expert stated that he should expect substantially equal rise in temperature in each part of the apparatus. He means the Pielsticker apparatus. What do you say in regard to this statement?

A. This must be wrong, because once your cracking has started, you get relatively little rise of temperature, and Pielsticker mentions the carbon formation in his coils, showing that cracking is taking place in the coils, in the latter part of the coil. We can say that in the coil we will have a rise of temperature, if it is fed, say, at room temperature into the coil, from this temperature to a temperature, say, of 700 degrees. We would have a rise in temperature of more than 600 degrees, whereas, as the cracking continues in the drum, we would have a rise from 700, perhaps, of half a hundred.

Q. You might not have any.

A. You might not have any, and you might have as much [fol. 1340] as thirty or forty or fifty degrees, but not any more than that.

The Master: You start with your oil feeding into your tubes at seventy?

The Witness: At seventy.

The Master: Your whole object in sending it through those tubes is to heat it up to a temperature of 700° and you do?

The Witness: Yes, so there would be 630° rise.

The Master: Yes, and that is where your first application and your greatest application of heat is.

The Witness: Yes.

The Master: Then in your tank, or drum, as you call it, which is a heated drum, you design that for your reactions and hold it at a temperature fairly constant to 700? You say you might have a rise above that?

The Witness: Yes.

The Master: What difference does the promotion of the rise in temperature from 70 to 700 and from 700 to 750 make? What is the significance of it?

Mr. Rauber: That, I think, is simply this—that defendants contend that the oil goes into that drum, and its temperature is greatly raised, whereas in their processes it is

not, the temperature does not rise in their tanks. I think we can bring that out on briefs.

The Witness: I was attempting to bring out that the statement that there is an equal rise of temperature in each part of their apparatus was incorrect, that there is not an equal number of degrees of rise in temperature in the drum as there is in the coil. Most of the rise of temperature is in the coil.

[fol. 1341] Q. Does the mere fact that the drum is heated necessarily indicate that there is a rise in temperature in the Pielsticker drum?

A. No, not necessarily.

Q. Further, on, defendant's expert gave the temperature of the retort as 700 to 750, and that the oil would be heated to around 400 degrees in the coil. Do you find any basis for this assumption?

A. I find that the Pielsticker U. S. Patent, Exhibit No. 134, gives quite the contrary. In other words, he indicates that the coil cracks the oil just the same as we have had indicated for the Holmes-Manley and the tube and tank process. At page 1670 of the exhibits, line 100, he states:

"In this case the cover 'F' can be easily removed and the pipes be occaisonally cleared of the carbon deposited inside from the heated oil by simply forcing an iron rod through the length of each pipe forming the coil."

This is clearly an indication of cracking, because otherwise there would not be carbon deposited inside the heated coil. It is the same process that is used for clearing the pipes in the tube and tank process and the Holmes-Manley and the Cross processes, and for the same reason, and in each case we have cracking in the tubes.

This would mean that we had reached a temperature of cracking which would be far above 400 degrees F., and would mean a temperature in the range, as he has suggested, of 700°.

The Master: Are "vaporization" and "cracking" necessarily synonymous?

The Witness: No. You might readily boil your liquids and not produce carbon, but if you produce carbon, you must have had cracking.

[fol. 1341½] Q. That would be particularly the case where you distill under pressure, would it?

[fol. 1342] A. Yes, it would make it much less easy to boil it out and make the cracking effect show up better, or more.

A. Yes, it would make it much less easy to boil it out and make the cracking effect show up better, or more.

Q. On page 2173, speaking of the Pielsticker process shown in Exhibits Nos. 133 and 134, defendants' expert states that in Pielsticker's normal operation there is no mention of pressure. What is your opinion on this statement?

A. My opinion is that when he is operating for the production of gasoline or lighter hydrocarbons, he is operating under pressure. As supporting that I read from page 1671 of the exhibits, line 106:

"When it is intended to produce still lighter gravity oils, a valve 'b' may be placed between the outlet of the retort 'L,' and the condenser 'U,' which may be kept open only so far as to create a certain pressure of the gases inside the retort."

And then he gives two other methods of giving pressure which I will not quote at present here. In other words, when producing light gravity oils such as gasoline he wants to use pressure.

Q. On page 2182, Defendant's expert stated in regard to Pielsticker that if he had been talking about a revolutionary thing in the art, like distilling crude oil at 20 or 30 pounds pressure, as some others like Young did, he would have come out and said so. What do you say in regard to this inference?

A. The use of pressures of 20 pounds or 40 pounds, or even more than that, of 90 pounds, was already well known. For example, Young, Dewar & Redwood and Riebeck—Young mentions 20 pounds, and Riebeck also mentioned four to six atmospheres. It was already well known, and I would not think of it being revolutionary to omit a definite statement as to pressure.

The Master: How about omitting any reference to pressure?

The Witness: Omitting any reference to pressure——

Mr. Rauber: No.

The Witness: Or admitting any reference to a definite pressure. It was already well known to use pressure.

The Master: How about omitting any reference to pressure? Wouldn't you assume if a man omitted any reference to pressure that he——

The Witness: That he did not mean pressure. But Piel- [fol. 1343] sticker does specify pressure here. In this connection it is interesting that Burton, in his original application did not mention a definite pressure. That is, it is just in the same sense as Pielsticker in this connection.

The Master: But he did mention a range of pressures.

The Witness: Not in his original application. That was afterwards introduced. In other words, his original disclosure of pressure was equivalent, or rather the same as that of Pielsticker.

Q. On page 2183, Defendants' expert stated:

"There was no gasoline art in 1892 that I know of, except the tube cracking processes, no considerable pressures were used. The practical art in the oil industry was all atmospheric pressure, that is up to 1912 or thereabouts."

Is this statement correct?

A. No. Riebeek was using a commercial process, was getting gasoline; he was operating under pressure, though condensing at atmospheric pressure. He was interested in making both kerosene and what we would now call gasoline.

The Master: What did he call it?

The Witness: He called it benzine. Young's process was used and made kerosene and gasoline as well. Both of these belong to the gasoline art of the early times and were operations under pressure.

Q. On page 2183, Defendants' expert stated:

"In other words, Pielsticker shows that if he wanted to make gas or very light oil, in other words, to do heavy or extensive cracking, he first had to get rid of the tar in the still."

Further on defendants' expert said:

"By inference I think that means that he could not produce that cracking practically in his other forms of apparatus."

What is your opinion in regard to this statement?

A. He only used his second coil—that is, the coil, retort and second coil—he only used the second coil, as he says, at page 1671 of the exhibits, for the manufacture of permanent gas or very light oil.

The Master: You are not quoting now. You are changing the wording, are you?

The Witness: Perhaps I had better quote. The exact wording is:

"If the manufacture of permanent gas or of very light oil [fol. 1344] is intended, the retort 'L' is directly connected with a second coil 'D'' similar to the first one 'D'."

When he was cracking but making not a permanent gas or extremely light oil, he used the coil and tank as mentioned in line 106 on the same page.

The Master: What do you understand by the term "extremely light oil"?

The Witness: There was about this date a small market, a moderate market for very light oil—that is, oil that would vaporize to a very considerable extent around room temperature. There was a market for that, for two purposes, one for use in ice making and again one for use in carburetting gas. Then you could even use and there were used some higher boiling oils for that purpose.

The Master: Illuminating gas?

The Witness: To make illuminating gas, by bubbling air through it. In other cases simply by adding it to a poor illuminating gas, in the same sense as adding it to air, and you would have a gas. They used for that a gasoline of a Baume of, say, 85 or 87. So it was a very volatile gasoline.

The Master; Then his extremely light oil was really a liquid gas?

The Witness: Almost. In summer weather it might be very largely, and in winter weather a fairly stable liquid. But when he wanted to make a gasoline he did not use that process, but used the process described in lines 106 on.

Q. That is, for making a light oil, you say, or what?

A. For making gasoline he speaks of still lighter oils, after he has been discussing some medium oils obtained by cracking.

Q. Page 2184, Defendants' expert said in reference to Pielsticker's apparatus of Exhibits 133 and 134 that, "in other words, either the column still W is connected with the outlet of the heating coil or with the outlet of the still, and one clause says one thing and one says the other." What do you say in regard to that statement of defendants' expert?

A. There are two processes disclosed, not a single one. They are alternative processes disclosed.

Q. Apparatus, you mean, don't you?

A. Apparatus—arrangements of apparatus for processes.

Q. Further on pages 2186 and 2187 defendants' expert says, in speaking of the column stills of the Pielsticker device of Exhibits 133 and 134 "in addition to that there is no indication that it is a bubble tower." Is that statement correct?

A. In the first place it is described as a column still, and column stills in general are bubble towers, and this is further indicated by the construction of the tower W. The fact that it is made as plate and cap bubble towers are ordinarily made, and as probably no other type of towers are made, of cast sections, as is indicated by the figure W, in which we would have the thick plates separated by the sections between the flanges, and in the way that bubble towers are customarily assembled the various sections being bolted together on the flanges here——

Q. Is that the conventional way of representing a bubble tower?

A. Yes, sir, it is the conventional way of representing a plate and cap bubble tower. That is well illustrated in the case of Thorp's "Outlines of Industrial Chemistry", in which a similar bubble tower is shown with the plates and [fol. 1345] base indicated. On pages 9 of Thorp's "Outlines of Industrial Chemistry."

Q. Have you photostats of that, doctor?

A. Yes (handing counsel).

Mr. Rauber: I offer in evidence the title page, and pages 8, 9, 10 and 11, of Thorp's Industrial Chemistry, Edition 1909 referred to by the witness, and ask that it be marked Exhibit No. 604.

(The said exhibit so offered was marked Exhibit No. 604, read in evidence, and the same is herewith returned.)

On page 2187, in comparing Pielsticker's apparatus of Exhibits 133 and 134 with the disclosures of Clark Patent No. 1119498, Exhibit No. 177, Defendants' expert stated with reference to Pielsticker:

"It really bears no resemblance to the Clark tube still cracking process, because Clark's essential feature was the use of an unheated drum and the re-circulation of oil from and to the bulk supply, where the cracking is taking place, neither of which Pielsticker showed."

How does this essential feature of an unheated drum in Clark's process compare with the Holmes-Manley process described by Defendants' expert?

A. It is my understanding that once the Holmes-Manley starts, they are heating both the drums and the coil, and in that respect the Holmes-Manley is not the Clark, in which the drum is unheated. Pielsticker, of course, also shows that this recirculation of oil from and to the bulk supply, in the sense that the run-back is run into the supply of oil and then passed through the tubes and tank again.

Q. Do you find the mention of salt water in the Pielsticker process, as described in "Exhibit Nos. 133 and 134" to indicate the application of this process to crude oil?

A. No, I do not. He mentions that the apparatus may be used for salt water, as I recall it. While crude oil might contain some salt water, shale oil would be free from any salt water content.

Q. What crude oils would contain salt water?

A. Many crude oils come up from the well mixed with salt water, and if they were not properly settled, the salt water being allowed to drop out, and the oil, free from salt water, decanted off, these crude oils might carry salt water.

Q. Of what oils is that particularly true?

A. I know it is true of our Atlantic oils—meaning by that, the Allegheny groups; and it is true of Mexican and of midcontinent oils.

Q. And California oils?

A. Yes, I think it is true of California oils.

The Master: I know it is true of Indiana oils. I have seen it.

[fol. 1346] Q. On page 2191, the defendants' expert described the leading off pipe from the safety valve, of more

than 60 feet—say 150 feet long—how do these dimensions compare with the apparatus shown in the Atwood patent, "Petitioner's Exhibit 80"?

A. That shown in "Petitioner's Exhibit 80," might be as long as this—that is, it is conducted through the roof of the building, and if the still was as large as that of the present day cracking still, it might easily be considerably more than 60 feet—that is, the height of the building, in, say, a Holmes-Manley still, is decidedly more than 60 feet—judging from the model which was exhibited at 17 Battery Place.

Q. Do you understand Atwood's pipe to be just sufficient to bring the end of it through the roof of the building?

A. No. He says, on page 1165, near the bottom of the first column, "Carried up through the roof of the building, or in any suitable maner, away from the vicinity of the still."

Q. How does that compare with the model of the Holmes-Manley process that we saw?

A. In that particular model the safety pipe was carried up, and not through the roof of the building, but simply projected under the eaves of the building, and anything ejected from it would hit the eaves of the building and be reflected down into the plant—down towards the ground.

Q. But the position of the end of that pipe is approximately the same as that in Atwood's patent?

A. In the case of the Atwood patent he says it goes through the roof. In the case of this one, it should have gone through the roof, but that model actually made it come up to the roof, and when the content would be ejected from it, it would hit the roof.

Q. On page 2192, defendant's expert stated:

"Atwood, for example, provides an auxiliary condenser in which the safety valve blows, and trys to condense a considerable proportion of those vapors."

Do you find this statement to be correct?

A. I do not.

Q. What is the fact, in the Atwood apparatus?

A. He leads his pipe away from his stills as promptly as practicable—that is, he leads it horizontally away from his stills, and then in a larger pipe, which has a vertical pipe

attached to it, leading through the roof. The horizontal pipe to which the first horizontal pipe leads, is spoken of as a main pipe, or condensing pipe, but it is not arranged as [fol. 1347] a condenser. It would catch only that which might come out and be chilled by air cooling. This would, of course, be true in a safety pipe, as shown on the model of the Holmes-Manley process, which we examined at 17 Battery Place.

The Master: What has the Holmes-Manley pipe go- to do with it?

The Witness: It was said to be an illustration of the Burton safety valve and pipe leading away from the safety valve.

Q. Would you consider any of these pipes in the Atwood apparatus, to be condensers?

A. No.

Q. On page 2193, defendant's expert gave as one of the principal functions of the Clark high line device, that it makes up any temporary deficiency in the amount of gases which would be produced in a given still. Was there anything disclosed in the prior art that would accomplish this same function?

A. Yes. The disclosures in the Dewar and Redwood patent would completely disclose a process of accomplishing this same function.

Also, in my testimony of more than a year ago, I gave certain other alternative methods which could be used.

Q. On page 2195, Defendant's expert, speaking of patent No. 1,129,034—the high line patent—stated:

"The greatest advantage of the Clark high line over the scheme of Dewar and Redwood, is the advantage of a single point control, due to having a whole battery hitched up with only one point for pressure regulation."

Q. How does the Dewar and Redwood arrangement compare with this?

A. It is similar, but not quite so convenient, inasmuch as several valves would be needed to be operative.

Q. On page 2207, defendant's expert stated:

"The principal draw-back in the method of Burton and Clark's patent, No. 1,448,254, 'Exhibit No. 195,' was that

while it did insure a stock going direct through the tube somewhat cleaner it was still mixed with a considerable quantity of dirty stock, which had to be re-circulated in order to keep the temperature at a rapid cracking temperature."

[fol. 1348] How does this compare with the operations of the processes of Schuchow et al., patent, "Petitioner's Exhibit No. 129"?

A. It is substantially the same.

Q. On page 2222, defendant's expert stated:

"In the Holmes-Manley process, Holmes found it necessary to limit himself to very light firing of the stills, and particularly the making of such disposition of the flames in that chamber, that there was no chance of impinging any of that flame, or very hot gases, on the stills, but, instead, a very gentle soaking heat, entirely surrounding the stills."

How does this compare with the heating in the vertical chambers of the Adams patent?

A. This, of course, is no near relation because Adams used extremely high temperatures—incandescent temperatures.

Q. On page 2240, Defendant's expert stated that in Schuchow, et al., patent, "Exhibit No. 129," the condensation, due to radiation, is accelerated by introducing the feed, at that point"—referring to a large drum at the top of the apparatus —"into the above container, which cools the material still more."

How does this compare with the operations of the tube and tank process, having a cold feed, as shown in "Exhibit No. 413"?

A. This is also true in the tube and tank process, "Exhibit No. 413."

Q. How do you mean, it is also true?

A. That is, introducing the feed into the bulk container cools the material. That is, in both cases the processes are essentially identical. We have in each case the heating of the oil in a coil, the passing of it into a bulk container, running into the bulk container of cold oil, a small amount of cold oil, and the allowing of the vapors to distill off.

Q. In your cross-examination you are criticized for saying that the receiver K of the Kittle drawing shown on page

1642 of the exhibits is a closed container, inasmuch as it shows just a single line across the top of the container. How was the type of that tube and tank device shown in Exhibit 413?

A. It is shown as a single line. This is a tube and tank. They criticized my direct and said it was without pressure, because it had only a single line in Kittle. I stated that it had pressure. In this case it has only a single line, though they are claiming a pressure of 350 pounds per square inch.

The Master: Yes, but that does not affect Kittle any.

The Witness: One as much as the other.

The Master: Kittle said he did not have the end of the pipe closed at all.

The Witness: Your honor, that is incorrect. He had it closed; he had it leading into a vessel which was closed, just the same as this one.

The Master: I mean beyond the sealing by the liquid, that is all.

The Witness: Well, that is all that this is, too.

The Master: And the amount of pressure that you could raise on that sealing of the end of the pipe was comparatively small.

The Witness: This is exactly the same case. The pipe is closed only by the pressure of the liquid, unless you take this line as meaning a cover in both cases.

The Master: In that drawing there?

The Witness: Yes.

[fol. 1349] The Master: Oh, we know what that line means there. The drawing was testified to. In Kittle we are speculating as to that.

Q. On page 2248 the defendants' expert stated in the Cross process practically no cracking takes place in the tube, or as little as possible. Is this statement true?

A. No, this statement is not true. I have analyzed the product coming from the joint, coming from a point between the tube and the tank in the Cross process, and I find that more than half or as much as half of the cracking has occurred at that time. Furthermore, that was a cracking of a kerosene distillate, and accordingly if we would use, as has been indicated here, a temperature of 850°, which was the same temperature used there, and a gas oil, we would have even more than half of the cracking occurring in the tube before it reaches the re-action chamber.

Q. What kind of stock were they cracking when they made those tests?

A. The tests that I made were with kerosene stock, which cracks even less readily than a gas oil stock—that is, less rapidly.

Q. At pages 2249 and 2250 defendants' expert stated that,

"The essential feature of Palmer was in the disclosure that it was possible to bring about cracking in the entire absence of distillation, by merely sealing material up in a vessel and exposing it to the high temperature. You may recall my pointing out, in discussing the prior art, the prevalance of the wild notions of the relation between distillation and cracking and between dropping back and radiation from hot surfaces, and how confused the art was. Palmer was really the first man who pointed out that cracking could and did take place in an entire absence of distillation and that you could simply seal up material and heat it up and get very considerable decomposition."

Is this statement of defendants' expert true?

A. In this connection it is worth while to discuss a little bit the old art. We do not have such dropping back and——

Q. In that connection, doctor, I would like to call your attention to the Benton patent, and particularly to the discussion of it in the book of Redwood's on "Petroleum." It is exhibit No. 107.

A. Benton in 1887 used a quite different type of process from that, and it would be impossible to consider any ques- [fol. 1350] tion of dropping back and radiation from a heated surface. There is nothing of that kind in the Benton process.

Q. How does it compare with Palmer?

A. A part of the description—I refer particularly to page 1823—seems to be such a process as Palmer.

Q. I mean as described in Exhibit No. 107, particularly.

A. Exhibit No. 107 does not give it as clearly, I think, as Exhibit No. 150 does. Line 83:

"Under this pressure the liberation of any vapor from the highly heated crude liquid is prevented until the heat shall have been continued long enough and raised high enough to bring the entire body of oil confined in the chamber or pipe or vessel to a uniform temperature, when it is next discharged into a spray into a vapor chamber."

Q. Do you understand the cracking to take place before the oil is discharged?

A. Yes.

The Master: Why?

The Witness: He so describes it elsewhere in that same exhibit, and it was so understood also by Redwood in his description on Exhibit 107, that it was a cracking process to get gasoline and kerosene.

The Master: That does not meet the question as to whether the cracking and decomposition took place in the tube or the vapor chamber. You say there may be vaporization without cracking. The question is, did the reaction take place in the tube or in the chamber.

The Witness: Redwood says at page 1386: "The process consisted in pumping the refuse of petroleum oil, tar oils and other heavy mineral oils, under a pressure of 285 pounds or more to the square inch, through a series of pipes heated in a furnace and allowing the products to escape into a vapor chamber."

In other words, they were products produced by this process.

In Eisenman——

Q. Eisenman is not a pressure distillation, is it, doctor?

A. No, but I do not understand——

The Master: You see, Doctor, here is what you have to keep bearing in mind all the time: You cannot take two or three prior art references and patch them together and make the process of the patent in suit, and your prior art reference must actually disclose and clearly disclose the same thing as the patent in suit in order to anticipate—not what might, could or would have been done if some change had been made. It must be a clear and distinct reference, or it is not an anticipation. The burden is upon the petitioner to show that.

The Witness: The question, your Honor, is not a question of anticipation directly, it was on the question of the accuracy of this quotation. Young and Thorp's apparatus was a digestion process, essentially, without distillation. That was an anticipation. The statement of defendants' expert that the invention of Palmer is found in its broader aspects in the re-action vessels of the tube and tank and in

the Cross process, must lead us, then, to the interpretation that the Benton process becomes also an anticipation.

The Master: If that were clearly shown in Benton and pointed out, and unless Benton comprehended it and stated it.

The Witness: One other thing I should call your attention to. That is, that Palmer gives as his temperature here 300 degrees centrigrade as an active cracking temperature, and 300 degrees Centrigrade is 572 degrees Fahrenheit, which is lower than the defense has claimed was a possible operative commercial cracking temperature.

Q. How do the conditions described in the Palmer patent compare with those in the tube and tank and the Cross and the Holmes-Manley processes?

The Master: Assuming 500 plus temperature may produce some cracking, and assuming that one of the patents in suit—for instances, the Burton patent—takes in somewhat higher temperatures than that, would that partial cracking be a commercial process at all? It might be merely incidental. I can conceive that it might possibly occur at low pressure distillation by some sudden rise of temperature or something of that kind and yet not be a commercial process at all and not come within the disclosure of the instant patent. Might it not?

The Witness: It might.

[fol. 1351] Q. In the Benton process as described in Exhibit No. 107, would there be any cracking in the tube?

A. There would be.

Q. That is before the pressure is released?

A. Yes, practically all of it.

Q. And do you understand there would be any vaporization in that tube, any distillation? I am referring to Exhibit 107. What I want to get is, does the vaporization take place before or after you release the pressure?

A. Primarily after you release the pressure. There might be and probably would be some vaporization before, depending on the temperature to which it was heated.

Q. How do the conditions described in Palmer compare with those described in the tube and tank, the Cross and the Holmes-Manley processes? Would there be any vaporization in the Cross, the Holmes-Manley and the tube and tank?

A. There would be, because in each case they were using temperatures of 800 or 850 degrees Fahrenheit, which are above—in fact 850 degrees temperature is above the critical temperature of the gas oil.

Q. Did you find from actual experience that there was some vaporization in the re-action chamber of the Cross process?

A. Yes, I carried out tests on the products in the Cross process at various stages during the process and found that there was decided vaporization from a point well before it left the coils. In fact, the material was distinctly above its critical temperature; that is, it was all—excepting for carbon and a few small amounts of such material as that—in vapor condition at the point where it left the coil and before it had entered the re-action chamber. In the re-action chamber it was essentially in vapor condition, and though it cooled down before it reached the final end of the long re-action chamber, it was partially in liquid form, probably largely as a heavy fog.

Q. What would be the effect of using a lower pressure than used in the Cross process.

A. Vaporization would take place even more readily. What I said regarding the Cross, would apply, of course, equally to the tube and tank. The Holmes-Manley used a temperature fifty degrees less according to Exhibit No. 411, so that there might be less vapor in that case.

Q. On October 14th, defendants' expert performed an experiment with an incandescent wire emersed in Nujol to discredit your statement with respect to the incandescent graphite emersed in oil in the Adams' processes. What can you say in regard to this experiment?

A. It was scarcely a fair test. The ratio of the size of the wire to the volume of oil being heated is nothing like the ratio of the incandescent portions described in the Adams' patent to the volume of oil being heated. The wire [fol. 1352] heated was a very small one, one in fact which it was difficult to see until one got very close to the beaker holding the oil; in other words, a wire but little larger than a hair, and the volume of oil was many hundred times that diameter, being approximately three by three inches—that is, in the beaker filled, three inches deep and three inches in diameter. In the case of the Adams patent, the incandescent batch of graphite occupies the greater portion of

the vessel, and we would not have at all the phenomena present in the beaker if we heated our graphite in the Adams process as carried out in accordance with his patent.

The Master: Did you understand that that demonstration was intended to represent the Adams process or that it was merely demonstrating some physical action which the witness said took place where you emersed a hot wire in oil?

The Witness: I understood that this was to discredit my statement that in the Adams process, using an incandescent batch of graphite heated by electric current, that it would be essentially vapor, only vapor, about the graphite; whereas, by using a very small wire and a very large amount of oil, relatively, it had the appearance as if it would be a liquid phase process, whereas in the case of the Adams patent it would be heated to such a temperature that it would be in the gas phase at that point.

Q. Defendant's expert also contended that you were incorrect in stating that graphite would be unsuitable as a material of construction for pressure stills, among other reasons because of its porosity. What do you have to say in regard to this statement?

A. Graphite is quite porous—in fact, fully 30 per cent of its volume is, under ordinary conditions, air, and accord- [fol. 1353] ingly, if we filled a vessel made of graphite even without pressure, with oil, we would gradually have the oil oozing through the vessel. If we put pressure on it it would go through much more rapidly—perhaps as rapidly as water would go through a thick piece of blotting paper.

The Master: It would make a very good filter?

The Witness: Yes, a very satisfactory filter. And if we would attempt to heat such a vessel, the oil would catch fire on the outside, and burn, and, in addition to that, the oil within the walls of the vessel would crack, and we would have gas forming, and parts of the graphite breaking off. So that this vessel would go to pieces within a short time after heating began, and fail to support any oil, at all.

Q. On page 2261, defendant's expert stated that Adams was the first man who, in a bulk distillation process for

making gasoline, disclosed the indirect heating of the bulk supply of oil in a continuous process of this type.

Is this statement of Defendant's expert true?

A. By indirect heating I understand he means heating by means of a coil, and with that understanding—and I believe he has so defined it—his statement is not correct. We have just this kind of a scheme described in the Pielsticker, Kittle, Schuchow and Gavrilow, all of which are exhibits of the Petitioner in this case.

Q. How do the disclosures of this feature in the Adams patents, No. 1,320,354, "Exhibit No. 185," and No. 1,327,263, "Exhibit No. 188," compare with the disclosures of Schuchow, Kittle and Pielsticker?

The Master: Wasn't that all gone over in your prima facie case, Mr. Rauber?

Mr. Rauber: No, your Honor, I don't believe we touched on that.

The Master: First point out the disclosures that are in those Adams patents.

A. In the Adams patent, "Exhibit No. 188," patent No. 1,327,263, in Fig. 1, we have the product in a pre-heater, *h*, which may be counted as the tube—it is also labeled 5—passing to the tank either by way of the pipe, 57, or by way of the pipe 16, so that we have, in this case, Adams feeding in his product from tube to tank, or from tank to tube.

Q. Which do you consider the tube, and which the tank?

A. In this particular portion we should not speak of the pre-heater, *h*, as the tube, but the part which is shown on the next page—the vertical pipe, 12, is the tube—we have [fol. 1354] the oil being fed in, either by means of the pipe, 57, to the tank, *x*, or down through the tube and out, or through the pipe 16, through the tube, up into the tank *x*—that is, he can feed, in his process, either way—tube to tank, or tank to tube.

Q. How does that tube compare with the tubes in Pielsticker and Kittle?

A. They are not near as similar to the tubes of the Holmes and Manley process, as are the tubes of Kittle and Pielsticker, or Schuchow and Gavrilow.

The same applies to "Exhibit No. 185," which is patent No. 1,320,354, and is, perhaps, best shown in Fig. 2, where

the oil may be passed first through the tube carrying the incandescent electric heater, up to the tank above, or may be fed, in an equivalent manner, apparently, through the pipe 41, to the tank, and then down through the tube.

In the Holmes-Manley, and other processes which are under discussion here, they are much more like the tube and tank processes of Pielsticker, Schuchow and Gavrilow, and Kittle, than they are like Adams—in either of these patents.

Q. Suppose you refer to Fig. 1 of patent No. 1,327,263, "Exhibit No. 188," and consider the heater, *h*, to be the tube and the other apparatus to be the tank. Where would the main amount of heating be—in the tube or in the tank?

Mr. Wiles: Your Honor, I don't see where the interpretation comes in, at all, or where it is material. The witness has just stated the interpretation.

The Master: The patent, itself, describes the functions of each of these things, and you are not to suppose that they are to be for anything else.

Mr. Wiles: I will withdraw the objection. Go ahead.

A. He describes this on page 2018 of the exhibits, line 98:

"A pipe, 9, connects the supply tank, 1, with the oil heater, *h*, in any desired manner. Then, from the heater the heated oil is conducted through the pipe, 11, to the converter, *b*."

In other words, it is not supposed to be cracked in the heater, *h*, at all, but only in the converter, *b*.

Q. Would you put most of the heat in the heater, *h*, or into the converter, *b*, in operating it under those conditions?

A. Under those conditions you put it, of course, into the converter, *b*.

Q. In that case you would have both the coil and the tank heated, would you not?

A. Yes, sir.

Q. And most of the heat would go in the tank.

[fol. 1355] A. Yes, sir.

The Master: Why do you assume that?

Mr. Rauber: Assume which?

The Master: That most of the heat of the oil goes into the heater, *h*?

The Witness: Because, your Honor, here he states, in the first place, that the oil heater, *h*, may be heated in any desired manner and then he states:

"It is best to heat the oil before it is admitted to converter, B, so that the heat from the furnace of the converter may be conserved for the more important function of cracking, splitting the molecules of, and rapidly vaporizing the oil."

If you are going to do all your cracking and your vaporizing—that is, get your latent heat, and your heat of cracking—those two heats are going to be more than the heat of simply warming it up.

The Master: Why, of course, that is obvious.

Q. Would the use of an incandescent temperature on the tank, have any bearing on that question?

A. Yes, the fact, again, that you use—that this is at an incandescent temperature——

The Master: I thought you were implying that there was something approximating the cracking temperature, in the oil heater, *h*.

The Witness: I don't think there is. The defense has used—the testimony of the defendant's expert has sometimes implied that there was.

The Master: I never got that, if he did.

Q. In any event, you understand that most of the heat goes into the tank.

The Master: You would have to contradict the patent, itself, if you did do that. I didn't understand Mr. Wilson to put himself in that position. It is obviously preheating the oil up to a temperature, with less waster of heat in the converter, itself.

Mr. Wiles: That doesn't mean that the bulk of the heat doesn't go into the pre-heater.

The Master: I don't know what you mean by the bulk of the heat. In other words, you mean, by that, that in order to get a cracking temperature you must be above five hundred, and to heat your oil in your pre-heater, to, say 400, and 400 is the larger fraction of 500?

Mr. Wiles: Why, no, your Honor, I don't mean that.

[fol. 1356] You would go, probably, to 700 in the pre-heater, before you would get appreciable cracking.

The Master: But you don't go to that, under the teachings of this patent.

Mr. Wiles: I say you wouldn't go to the cracking point, but you would go as near to it as you could. You would have to raise your oil to from 500 to 600 degrees in the pre-heater. Now, contrary to Dr. McKee, the heat required for cracking is very slight. You only have to just raise it and hold it.

The Master: Under the circumstances, you assume that would be true?

Mr. Wiles: Yes, sir. If you get close to 700 you can carry it to 750, and——

The Master: In other words, somebody might do that?

The Witness: But it is not distinctly pointed out in the patent, that that is what is intended.

Mr. Wiles: I think it is quite indefinite, just what his ratios were.

Q. On page 2261 the defendant's expert said that Adams was the first to disclose in a bulk pressure distillation process for making gasoline, the condensation of the gasoline distillate and separation of the gas therefrom, under still pressure. Is this statement true, or do you find the same thing in prior art patents that you have discussed in this connection?

A. Dewar and Redwood do it. Lang does it.

Q. Will you point out the exhibit number?

A. Lang, "Exhibit No. 135," Dewar and Redwood, "Exhibit No. 103."

The Master: All those references are pre-heating, to some extent?

The Witness: Oh, no, your Honor. We are leaving that. The question, itself, must be answered within the wording of the question, though I presume it, primarily, is concerned with pressure condensation, but as the statement made by the expert, as that all atmospheric stills had it, because they are condensing at the same pressure at which they are operating.

[fol. 1357] Q. At the same pressure as in the still, you mean?

A. Yes, sir.

Q. In the tube and tank, and Holmes-Manley processes—

The Master: Of course, in an atmospheric pressure still you are condensing at the same pressure that you are distilling, which is no pressure, at all, and that has no reference, at all, with respect to pressure.

The Witness: As I said, my feeling was that he meant the other, but still the wording——

Q. In the tube and take process and in the Holmes-Manley process, do they condense under the same pressure as in the still—that is, under still pressure?

A. The tube and tank process does its cracking at a pressure of, roughly, three hundred and fifty pounds, and it is then reduced to 60 pounds, to pass into the separating tank, and then reduces further to 50 pounds, to relieve the gas from the condensed gasoline, so that they are at different pressures.

The Master: Yes, but they are all under pressure.

The Witness: But the relief of the gas from the gasoline is not under the same pressure as the cracking process in the tanks.

Q. As the still, you mean?

A. As the still—it would correspond to the still. In the Holmes-Manley the still has been divided into several parts—four—as shown on "Exhibit No. 411," and the pressure there, is about 500 pounds per square inch, and then it is reduced to 300 pounds, and then later on, to 80 pounds, so that it is not at the same pressure as in the still.

Q. On page 2261, Defendant's expert also stated that Adams was the first man who, in bulk distillation processes for making gasoline, disclosed the feeding of fresh stock, and drawing off pressure tar continuously.

Is this statement correct?

A. Dewar and Redwood disclose such a process. Pielsticker discloses such a process. Also Schuchow and Gavrilow and Kittle and Lang uses it as an optional method. All of those are exhibits which are before the Court.

Q. The defendant's expert also stated that Adams was the first man who, in a bulk pressure distillation process, [fol. 1358] for making gasoline, disclosed the application

of heat to the intermediate portion of a vertical cracking still, below the liquid level.

Is this statement correct?

A. I don't so consider it. I consider that Adams does his cracking in the lower part of these figures——

The Master: That is not the question. The question is something different from what you are answering, and I am very much interested in that question. Cite your references. It is a very simple question. If you have any references in the prior art, where that is done, point it out.

The Witness: In the first place, I don't believe Adams did it.

Q. Leaving that aside—do you find it in Schuchow, et al. "Exhibit No. 129"?

A. Yes, I find it in Schuchow and Gavrilow, and Warden, at atmospheric pressure, did it.

The Master: But the question is about a pressure still.

The Witness: Well, it is in Schuchow and Gavrilow and Kornfield.

Q. Will you point it out in Schuchow and Gavrilow—the parts that are heated—that is "Exhibit No. 129"?

A. I have found, in this figure given on page 1621, a series of coils, which, in the text says may be replaced by straight pipes. We have the heating of the tubes here, below tank, *a*, all of which carry oil.

So that we would have the heating of this portion—meaning the tubes, *d*—which would be below the liquid level in *a*.

Q. That is, if they were straight tubes?

A. Yes, if they were straight tubes, as described in the—he says that they may be changed to straight tubes.

The Master: Well, then, that means that any coil would anticipate this Adams disclosure.

The Witness: In the case of Schuchow we have a tube below, and it has been claimed here that the tube where the heating is done in Adams, is also a—sometimes a tube, or a tube and tank, and then, again, as a part of the still, for the other—that one is as much a still as the other.

Q. In making that comparison do you understand those tubes to be vertical?

A. Yes, though they might be placed in some other direction. It doesn't say.

Q. Do you understand, as shown, that they are vertical tubes, heated intermediate?

A. ——.

[fol. 1359] Q. How are the tubes disclosed in that patent?

Mr. Wiles: I object to that as already answered.

The Master: Let him answer. I want to hear his full reasoning on that. If the reasoning is correct, any of these prior exhibits in which the coil is run through a lead bath, would anticipate Adams.

Mr. Rauber: The point I want to bring out is that in this particular case the coils run vertically.

The Master: So they do in most of the patents—that is, they run spirally, upward.

The Witness: It says, "the coils." There are several coils, you will note, in the figure. "The coils may be replaced by straight pipes," which would imply that if one coil was replaced by a straight pipe, it would be a vertical pipe.

The Master: Of course the object of the spiral coil is to keep the materials a long time within the heating zone. If you substitute a vertical pipe there you must, necessarily, shorten the time within which the material is in the heated zone.

The Witness: Or increase your fuel consumption.

Mr. Wiles: The natural thing is to use banks of horizontal pipes, of course.

The Master: And if that reference that you make there, answers to this description of Adams, so it is in the other coil shown in the prior art—the heated coil. That is your position?

The Witness: No, not quite. Here he said that this can be replaced with straight pipes. If you have a straight pipe——

The Master: Well, all right. We will say that he has already pointed that out and disclosed that—that the pipe may be vertical, and you say that assuming the alternative method, which he well points out in his patent, is the equivalent of the Adams disclosure, here—assuming the pipes to be vertical—so, if the vertical pipe is the equivalent of the coil, then, equally, every heated coil in the prior art would anticipate the Adams patent. Isn't that necessarily so?

The Witness: We have in the Schuchow and Gavrilow one additional element—that is, a widening of the coil to a tank, just as Adams does—that makes it even more parallel, and Adams is using an extremely hot temperature, an incandescent temperature here, for this, which Schuchow and Gavrilow have apparently done away with, by using a greater length of pipe.

The Master: Just look at that drawing, Fig. 2, in patent No. 1,327,263—that is something different from either Schuchow and Gavrilow's coil, or his vertical pipe. What is it that distinguishes that change of Adams, from the other?

The Witness: I should say that it was the heating to an incandescent temperature, of the short length of the bottom of the tank—which has been called, here, a pipe.

The Master: Intermediate of the liquid level?

The Witness: Yes, in the sense of part of the feed.

The Master: Exactly.

The Witness: But not as distinguishing it from the others, because the others heat in the same way.

The Master: Why, no—if it was comparable to the patent cited,—Schuchow and Gavrilow—the heating chamber would extend clear up to the point on *x*, in the vessel above—from the bottom clear up.

The Witness: I say the difference as I see it, is the heating for a short distance, to an incandescent temperature.

The Master: That is all he discloses, then—simply the application of the heat at the intermediate level—is that right?

The Witness: No, I don't think so, your Honor.

The Master: Well, then, all right. Instruct me, please. I want to know about this. Apparently, when you look at that drawing and try to conceive the structure of Schuchow and Vavrilow, and compare it with the drawing and description of this Adams patent, you feel a difference there, somewhere. What is the difference?

The Witness: Perhaps, in considering it, I am thinking of what has been described as a coil. Of course this Adams patent would be inoperative, as given, and I am thinking of, say the Holmes-Manley, or some apparatus of that [fol. 1360] kind. So that when one compares what is disclosed in the Adams patent, which we have been discussing,

and what has been disclosed in the Schuchow and Gavrilow patent, we find that the Schuchow and Gavrilow is much nearer the Holmes-Manley, so far as these parts are concerned and this operation is concerned, than is the Adams, and——

The Master: We are not concerned with Holmes-Manley, now, at all. We are concerned with Adams, right here, now—this disclosure.

Mr. Rauber: The question is, your Honor, of how broadly to consider Adams.

The Master: I want to know just how broadly we can consider Adams. If the Doctor can limit it down to the narrowest construction, I want to see him do it.

The Witness: In the case of Adams, the essential phase of that is the heating of this tube, which is indicated here as an incandescent temperature, he is heating at a way high temperatures—much higher than we use in ordinary cracking processes—and he must do it if he is going to crack, supplying all his heat, or the major portion of his heat to a very short length part of his apparatus.

In the Schuchow and Gavrilow, if we consider this, or these, as vertical pipes, giving them a diameter enough so that they will be the same from top to bottom, or from bottom to top—doing the same cracking that you would do with a coil—we would have something merely parallel to the Adams, and we would have even closer to what are said to be the applications of the Adams.

Q. On page 2261, defendant's expert stated that as far as he could find, Adams was the first man who, in a bulk pressure distillation process for making gasoline, disclosed the use of a plurality of vertical cracking stills, operated in series, with a common vapor line.

What do you say as to this statement?

A. Dewar and Redwood disclosed everything excepting the vertical still. Their's are horizontal. Kornfeld—

Q. "Exhibit No. 123"?

A. (continued). Kornfeld also had a common vapor line. He states in the text that the boiler is composed of a series of vertical generator tubes, or inclined tubes.

On page 1572, the drawing, however, puts the tubes in parallel, instead of in series.

The Witness: Intschick also shows a plurality of vertical cracking stills with a common vapor line.

[fol. 1361] Q. On Page 2261 Defendants' expert stated that as far as he could find, Adams was the first man who, in a bulk pressure distillation process for making gasoline, disclosed separate condensation and re-cracking that part of the cracked vapors which is heavier than gasoline. What do you say in regard to this statement?

A. Any process which was used for the making of kerosene would also make gasoline. That Dewar & Redwood process shows just this, returning the heavier vapors, probably part of the kerosene and certain parts of gas oil, to the tanks for re-cracking. Pielsticker returns to the supply tank and uses for re-cracking that portion of the condensate which was condensed out in the dephlegmator which he used. Riebeck at page 4230 states that he re-works his so-called middle oils obtained——

The Master: You do not mean Riebeck. You mean Krey, do you not?

The Witness: I mean Krey, that is correct, at Riebeck plant.

A. (continuing:) Schuchow & Gavrilow in their dephlegmator condense a part of their cracked vapors and send them back to be re-cracked when they run in the cold feed at the top of the tank A. Barbet re-cracks his condensate; Lang also does. These are all exhibits in this case.

Mr. Wiles: It would save a great deal of cross-examination if he would get his references ready and be prepared for the question.

Q. How does the method of heating in Pielsticker, Kittle, Schuchow and Vavrilow compare with the heating in the Holmes-Manley and the tube and tank processes?

A. Essentially the same. They are substantially identical.

Q. How does the heating in Adams compare with the tube and tank and the Holmes-Manley process?

A. Not at all closely.

Q. Which do you find closer to the tube and tank and the Holmes-Manley, the Adams' method of heating or that of Schuchow, Pielsticker and the Kittle method?

A. The Schuchow, Pielsticker and Kittle are much closer.

Q. On page 2263 the defendants' expert states:

"I have also discussed the application of heat to the drum in the Pielsticker process, and shown that Pielsticker's disclosure could only be fairly interpreted as applying a great deal of heat to the large drum, and that he, pre- [fol. 1362] sumably did not heat to a cracking temperature in his coil, but only went to around 400 degrees, and then the rest of the way by the heat put into the drum, assuming, roughly, equal firing in the two sections, as plaintiff's expert said he considered reasonable, in view of the similar furnace settings."

Do you find anything in the Pielsticker to justify this temperature of 400 degrees as the temperature of the coil?

A. No, he cracks in the tube and has to get out carbon showing that he considers the cracking a normal operation of his tube. That would mean a very much higher temperature than 400 degrees F.

Mr. Wiles: Oil temperature?

The Witness: A very much higher temperature than 400 degrees.

Mr. Wiles: For the oil?

The Witness: Yes.

Q. On page 2263 and page 2264, defendants' expert distinguishes Kittle, Benton, Schuchow, et al., and Turner, and says that in none of them "is there any relatively large body of oil which is kept heated by the discharge into it of a stream of oil which has been heated in a restricted passage." How do the disclosures of these patents to Kittle, Benton and Schuchow and Turner compare in this respect with that of the Adams' patents?

A. It takes a great act of the imagination to make a tube and tank out of Adams, whereas Kittle and Schuchow are obviously tube and tank, and the others are fairly close.

Q. In the Kittle and Schuchow, et al., devices is the large body of oil kept heated by the heat from the coil?

A. Yes, and the tanks themselves are mildly heated.

Q. On page 2267, speaking of the Lang patent No. 954575, Exhibit No. 135, Defendants' expert said that Lang

is really not trying to make gasoline. How does Lang's description of the product he is trying to make, compare with the description of the products sought in the Burton, Ellis and Adams patents, particularly that with reference to Ellis?

A. Lang is interested in obtaining an oil of relatively high volatility, one which would be highly volatile, even at low temperatures. Ellis wishes to obtain a gasoline fraction boiling up to 125 degrees C., or 150 degrees C. 125 degrees woud be 257 degrees Fahrenheit and 150 would be 302 degrees Fahrenheit. In other words, the products wished to be obtained by the two are of the same general character.

Q. Both would be very light gasolines, would they?

[fol. 1363] A. They would.

Q. On page 2268 Defendants' expert said that condensation under pressure in Lang could not possibly have the same function as in Adams or Burton. What do you find to be the disclosed function in each of these patents?

A. Lang is doing it to obtain a more volatile gasoline, and hence he condensed under pressure. Burton said that it was to change his olefines to paraffines, so that he will get a higher percentage or amount of paraffines, of saturated hydrocarbons. Adams said nothing as to the function of his condensation under pressure.

Q. What is the real function of the condensation under pressure?

A. It is to permit the increase of the light ends of the gasoline by taking up in the gasoline those portions which would otherwise go off with the gas—taking up a portion of those which would otherwise go off with the gas.

Q. Those are the more volatile portions, are they?

A. Yes. It increases the light ends of the gasoline, and makes it more volatile, just as Lang says.

Q. On page 2269, in speaking of withdrawing tar and cracking to make kerosene and withdrawing tar when cracking to make gasoline, defendants' expert stated that the function or utility of withdrawing tar is entirely different in the two cases. What do you say in regard to this statement?

A. I can see no difference. In fact, if we take such a case as that of Burton, where roughly half of your mate-

rial is distilled, and the other half goes out as tar, and Riebeck where you have approximately the same distillate and the other goes out as tar, one first having been divided primarily for making gasoline and the second primarily for making kerosene, I can see no difference so far as the tar removal is concerned. You get essentially the same tar and the same amount from a still working at the same pressure on the same raw material, but not condensing the distillate under exactly the same conditions.

Q. Would the comment you have made in regard to Riebeck apply to Dewar & Redwood?

A. Yes.

The Master: In any of these processes, then, the only [fol. 1364] difference in the withdrawal of the tar is what constituents you withdraw with your tar; is that right?

The Witness: Yes, to a slight extent, but actually you distill off kerosene and gasoline in the Burton, and you distill off kerosene and gasoline in the Riebeck and in the Dewar & Redwood. So that actually the composition is practically the same.

Q. On page 2271, speaking of vertical stills, defendants' expert states that it is very difficult to put in a substantial amount of heat in a vertical still. What do you say in regard to this statement?

A. It is interesting in this connection, that in the putting in of heat into the steam boilers, we have gone gradually from the horizontal fire tube boiler, and have worked more towards the vertical tube, water tube boilers, in order to be able to put more heat into them and to economize on coal. In this connection it might be stated that there are processes, commercial processes use which are actually using vertical tubes for cracking the oil.

Q. On page 2271 defendants' expert stated that the patents to Intschick, Gaster & Barbet of Exhibits 124 and 125, lack the feature of operation under pressure.

Do you find any mention of pressure and any indication of it in Intschrick or Gaster?

The Witness: I do.

Q. On page 2273, defendants' expert stated that Barbet's bulk pressure distillation of Exhibit No. 136 was de-

voted to very light cracking to make kerosene. What do you say in regard to this statement?

A. It was devoted to cracking for kerosene, but cracking for kerosene is always accompanied by an equivalent amount of gasoline. In other words, light cracking and heavy cracking is, I think, a phrase that is without basis here. There is no difference except slight difference in degree.

The Master: Well, there is a difference in product.

The Witness: No, both make a mixture of gasoline and kerosene.

The Master: There is no separation out of gasoline in the Barbet disclosures that were here on the wall.

Mr. Rauber: Not in that one.

The Witness: Not in that part which was drawn off, but there was in the patent of which that was a part.

Q. Do you find in the Barbet patent exhibit No. 136, any provision for recovering the gasoline produced in the cracking still?

A. Yes.

Q. Will you point out the element with which he recovers the gasoline? Just the number is all I want.

A. It goes through the upper towers and is condensed and goes out through the condensers D into the receivers 1, 3, 4 and 5.

Q. At page 2273, speaking of the Barbet disclosures of exhibits 124 and 125, defendants' expert said that, "he simply had a film of liquid running down the walls of the tube, and of course in any operation of that kind, where most of the cokes before it gets to the bottom, you have an entirely inadequate cooling of the tubes."

Q. Is there anything in these Barbet patents which indicate that most of it cokes on the walls of the tube?

A. On page 1581 he states that there will be part of the carbon attached itself to the tube, and part of it will descend into the reservoir G, if the feeding of the oil is greater than the disassociating power of the tubes. I think also that the same thing is shown in connection with Exhibit No. 125.

Q. On page 2275 defendants' expert stated that the great virtue of the scrapers in the Holmes-Manley process is in

the agitating action and the removal of the asphaltic material from the walls before it is converted into coke. Do you find any agitating means in the Barbet patent, Exhibit No. 136?

A. Yes. In the cracking still P there is an agitator.

Q. In your opinion do the scrapers of the Adams device appear to be suitable for accomplishing this agitating and asphalt removing function?

A. No, they are of such a character that they would clog and the carbon would go around with the scraper and stay [fol. 1365] with the scraper and you would have what would be in effect a solid rod twisting around in the part of Adams where the scraper is indicated.

Q. How does the Adams disclosure compare in this respect with the disclosures in the patent to Gaster, which is Exhibit No. 127?

A. The Gaster disclosure, page 1596, of Exhibit No. 127, is described as one which is much nearer that of the Holmes-Manley than Adams, in that "the radial arms 4, carrying the blades or scrapers, 5, through the intermediary of a hinged piece 6, (Fig. 8)."

In other words, they are hinged as are the Holmes-Manley ones, and as Adams is not.

Q. How does the patent to Intschick compare with the two Adams' patents, Nos. 1462677 and 1462678, as to the use of a number of vertical retorts connected together?

The Master: Mr. Rauber, what do you hope to accomplish by attacking one element and showing there is one element in the patent that is old?

Mr. Rauber: That is the only element in those two patents that is used—of those two patents. I may be mistaken. I will bring out that point.

A. They are practically identical. Intschick sprays in the oil, also so does Adams. They use a screw for the removal of the carbon.

Q. Who does?

A. Intschick uses the screw in one place, or optionally other forms are mentioned as well. But in one place he does use the screw form. He removes the tar by a common draw-off, as does Adams. Pressure in each case, in the two Adams and Intschick is mentioned, but the amount

not given—identical again. And indeed, about the only difference is that one uses a set of five and the other a set of six retorts.

The Master: In a battery?

The Witness: In a battery. Adams uses a multiple battery of a set of four, two in one direction and two in another; Intschick uses six in a circle.

The Master: Do these units operate in parallel or in series in either one or the other?

The Witness: They are operating in parallel in all three.

Q. On page 2281 defendants' expert stated that the bulk pressure distillation processes which were cited against the feature of recracking the heavy distillation in a bulk pressure distillation process were Pielsticker, Dewar & Red- [fol. 1366] wood, and Barbet of Exhibits Nos. 136 and 141. What do you say in regard to this statement?

A. There were a number of atmospheric citations, and in this connection it should be borne in mind that there is no fundamental difference by a pressure change, no more than walking with or without an overcoat. In other words, atmospheric pressure is 15 pounds pressure absolute, Burton uses 60 plus 15, that is 75 pounds pressure absolute; Young uses 20 plus 15, that is 35 pounds absolute, and so on. So that it is really a question of exact limitation only.

Mr. Wiles: There is no difference when you use a vacuum either, I assume, on the same basis?

Mr. Rauber: Wait, do not get into comments. Let the questions come from the Master or else through our counsel.

The Witness: Schuchow & Gavrilow should have been mentioned in this connection also, as it is——

The Master: As one of the original references, you mean?

The Witness: Yes.

Q. And how about Krey's process?

A. And the Riebeck process or Krey process is also a process which recracks the product.

The Master: The question was whether that was one of your original references.

The Witness: Yes, I think so. But my point is that he should have mentioned it and he did not.

The Master: He was only bound to answer such as you cited.

The Witness: Well, I cited it, I think.

Q. On page 2281 defendants' expert said that in the case of Pielsticker and Dewar & Redwood, the heavy distillate which they recrack consisted primarily of the heavy ends, and, further, that the material which they wanted to put back was only material in the gas oil range and higher. What do you say in regard to this statement?

A. I agree with this, but it must be borne in mind in this connection that the cracking for kerosene and gasoline are identical. It is only the distillation that can make a difference. None of the plants, either the old ones or the present ones, can make one product only; they must make the two products, and the process of making them is essentially the same.

Q. Would these heavy ends that are returned in Pielsticker and Dewar & Redwood contain some kerosene fractions or hydrocarbons in the kerosene range?

[fol. 1367] A. They would.

The Master: Going back to the next to the last question, if I understood you correctly in that statement, all of the processes of cracking in their essential successive steps, regardless of the apparatus used for the operation in each step, are essentially the same; is that right?

The Witness: Yes, and no, your Honor. So far as they apply to the cracking for gasoline and kerosene; not when they are cracking for permanent gas.

The Master: That is what we are talking about, kerosene and gasoline.

In other words, there are certain successive steps in the process of taking one of the heavier oils, gas oil or some intermediate oil and carrying out the process to get the product gasoline. The essential steps are all—three or four or five, whichever they are, and no matter what the apparatus you use for forming each of those successive steps, the process consists of so many steps and results in refined product, and they are all the same, the processes?

The Witness: So far as making gasoline and kerosene by heating of petroleum——

The Master: By decomposition and cracking.

The Witness: By cracking processes—the one that makes one will have to make the other, and they are in their fundamentals the same. In other words, if we heat to, say, 720 degrees, we will get gasoline, and if we do it as in Dewar & Redwood, we would get also gasoline, and if we do it as Burton does, we will also get gasoline.

The Master: In other words, the steps, as I understand you, in the process, are all invariably the same, the quantity of either kerosene or gasoline that you get will be determined by carrying your temperatures and pressures to higher points. Is that right?

The Witness: If you omit the cracking of the kerosene when making gasoline, the quantities will be almost identical also. That is, you cannot crack gas oil, for example, by varying your process, without producing approximately the same proportion of gasoline and kerosene, no matter what process you use, excepting for a recracking of kerosene, unless you go so high that you get a large proportion of gas. In other words, use a gas making process that gives you only gasoline as a by-product, or nearly so.

Q. Do you mean that you always produce some gasoline and some kerosene; is that correct?

[fol. 1368] A. That is always true. And the amounts you produce are not greatly varied.

Q. Then the function of returning the heavy condensates in all processes is fundamentally the same; is that correct?

A. Yes.

The Master: Let us get this clear. It is quite important in this case. The process we will keep clearly distinguished from apparatus or the mechanism from the process. And I understand you to say that the processes involved, the successive steps from beginning to end, are always and invariably the same, and you may have a difference in apparatus to accomplish a step, but the process will be the same?

A. In a major sense. That is, not going into detail, but in a major sense, so far as cracking by heat, it would not be the same as cracking by a catalytic agent, for instance.

The Master: No.

The Witness: But cracking by heat.

The Master: We have nothing but the cracking by heat in this case.

The Witness: Cracking by heat, the major problem is to heat it up to a cracking temperature, and when that is done we get approximately the same proportions of gasoline and kerosene—forgetting for the moment the recracking of any kerosene to get gasoline. We get approximately the same proportions of kerosene and gasoline, no matter which of the two processes we use, the old prior art, or the present-day processes.

Q. Of course, if you return the kerosene back and crack it further, you will get more kerosene and less gasoline?

A. That is correct. That is the primary exception as between the old art and the new art. That is, the old art was primarily concerned in making kerosene and getting it out; the new one wants to return it for recracking. They use the same kinds of processes, though.

Q. What is the function of the pressure in these processes, then?

A. The function of pressure is to permit the temperature to be raised without boiling off the material, and thereby speed up the operation. As I stated, an increase of twenty degrees Fahrenheit approximately doubles the speed of cracking, and accordingly, if we increase our pressure, any, from four atmospheres to somewhat higher—I do not know the exact figure, but any six atmospheres—we will probably double the speed with which we crack. These figures I am [fol. 1369] giving on atmospheres are only approximate, of course. Because we will have changed the temperature at which we will operate from, say, 710 to 730 degrees Fahrenheit, and that would mean doubling the speed of operation, the speed of cracking. And that is essentially the only purpose of pressure. There are minor advantages and a good many disadvantages of pressure outside of that.

Q. What would be the effect, Doctor, if you attempted to increase the temperature without increasing the pressure?

A. You would simply distill off your material faster through your condenser and find that you really practically could not increase your temperature due to the fact that the boiling off of the liquid would lower the temperature

due to the latent heat of evaporation. On the other hand, if you increased your temperature and also keep your escape for your gases only at the same point as before, the greater increase of the rate of cracking due to this increase of temperature would give you more gases and thereby increase the pressure, so that it would bring up the pressure in the retort.

The Master: And you reduce your return.

The Witness: What is that?

The Master: You reduce your return of gasoline by the escape of the gases?

The Witness: Yes, of vapors by the gases, unless those were washed or reworked through a coke tower.

The Master: The force supplied in this art is heat; the material to which the force is applied is one of the intermediate oils of petroleum. If your definition of process holds true, there would seem to be no room for invention in the field of the method of the application of the force to the material.

The Witness: I do not think that is necessarily so.

The Master: Is that so or not so?

The Witness: I do not think it is. That is, you can have not only apparatus, but you can have methods of getting higher temperatures, to give you increased speed of reaction—that is, such as the tube, and schemes compared with the tube and tank.

The Master: These schemes of getting increased pressure, do they or do they not involve inventions?

The Witness: They may.

The Master: And likewise, then, would you say that the differences in devices in respect to the method of the application of heat furnished a field for the inventor?

The Witness: Quite likely.

Q. In regard to Pielsticker and Dewar & Redwood processes, if the only material that you returned to your cracking operations was in the gas oil range or higher, would it be stripped of the gasoline hydrocarbons?

A. It would, if you mean by stripping that they be allowed to go on to the condenser.

Q. They would be stripped out of the heavy condensate, would they?

A. Or I would rather say that the heavy condensate would be stripped out from them.

Q. From the gasoline hydrocarbons?

A. They would be separated, yes.

Q. In a bulk pressure still, if the reflux were cooled and returned to the supply tank, rather than directly to the still, would the heat in the reflux liquid necessarily be lost?

A. It would not. You could use a heat interchanger of the kind that is commonly used in the industry, and this would recover the heat.

Q. On page 2286, defendant's expert stated that, as a result of the citation of the Danre patent in the Adams applications, after the affidavits were filed, the claims that were finally issued were all distinguished, either by including the feature of carbon removals, or by some limitations to the that those processes operated on oil and liquid form?

In the Lang and the Dubbs patents do you find any evidence that those processes operated on oil and liquid form?

A. Yes.

[fol. 1370] *A.* Yes, for in each case there were drawoffs for the liquid tar, from the bottom of the cracking vessel.

Q. In the Lang and Dubbs processes would carbon be drawn off with this tar?

A. Yes.

Q. On page 2297, defendant's expert, speaking of the Kittle patent, says:

"That inasmuch as there must be substantial input of heat into the drum, the drum temperature would necessarily be higher than the coil outlet temperature."

Do you find any evidence to substantiate that the drum temperature would necessarily be higher than the coil outlet temperature?

A. No. Kittle says that the temperature does not rise.

On page 1646 of "Exhibit No. 131," which is the citation, we find this:

"The latent heat of vaporization prevents the temperature of vapor from reaching that of the bath. To this end the quantity of this oil which passes through the apparatus is preferably regulated. The vapor continues to absorb heat, but its temperature does not rise to an appreciable extent."

Q. On page 2297, defendant's expert quoted you as saying that the temperature of the Schuchow, et al. drum, would be 400 to 500 degrees, which he says is far below a satisfactory cracking temperature. What do you say in regard to this statement?

A. The factors which were under discussion at that time, in my examination, were those at the top of the dephlegmator, and the soaking drum portion was below, and would necessarily be at a higher temperature. This is indicated clearly in my testimony on page 841, at the top of the page.

The Master: You refer to a soaking drum.

The Witness: The digesting drum.

The Master: That was what I was going to ask you—a reaction chamber.

The Witness: Yes, sir.

The Master: A reaction tank, a digestion chamber, or tank, and a soaking drum, are all the same thing, are they?

The Witness: They are.

The Master: Functionally?

The Witness: Yes, sir.

The Master: I am sometimes confused by calling the same thing by different names. That is all.

Q. In regard to the Ellis patent, No. 1,415,230, defendant's expert stated that in the drum of the Ellis device heat is used up by the cracking reaction, and also by evaporation. Is this true, also, of the Kittle and Pielsticker drums?

A. This is also true of the Pielsticker and Kittle drums, and was so described in the answer that I gave to a question a few minutes ago in connection with the Kittle patent.

Q. What effect would this absorption of heat have on the rise, or drop in temperature in each case—that is, in Pielsticker and Kittle?

A. It would tend to lower the temperature and with the drop of temperature there would be a decrease of the rate of evaporation, and also of cracking, so that it would tend to give a balanced action, to some extent. But the general effect is to lower the temperature, as both operations of evaporation and cracking absorb heat.

Q. If the drum is being heated, the effect would be what, in that case?

A. To balance the heat loss due to cracking and to evaporation.

[fol. 1371] Q. Do you mean the effect would be to counteract that of the heat absorbed in cracking and evaporation?

A. Yes, sir.

Q. On October 18th, defendants' expert stated that in discussing Aisinman and the Riebeck reference you had created a number of imaginary oils, which he compared to imaginary giants and imaginary dwarfs. What do you say as to this contention of defendants' expert?

A. I consider that the defendants' expert's comments in this, were quite unjustified.

Aisinman was working on oils of varied origin, and hence not with oils of a single family, such as defendants' expert implies. Aisinman obtained large and varying amounts of aromatic hydrocarbons. See "Exhibit No. 118," on page 1488.

Now, the aromatics, as has been testified to here several times, are heavy, but of low viscosity. Taking the exhibit as a whole, my previous statements are correct, and defendants' expert's colorful comparison is, I think, without correct basis of fact.

The Aisinman article, "Exhibit No. 118," shows that he obtained paraffins and aromatics and varying characters of oils—that is, that he used varying families of oils. The criticisms made would only apply in case of a single family.

The Master: What do you mean by a family?

The Witness: By a family I mean, if the oils were predominantly of one kind, all of one kind—such as all paraffins, or all naphthenes, or all olefins, but not when they are mixed, such as he has here, such as aromatics, and which are rather uncommon in American oils, in any quantity—and he has the aromatics, which are more dense and of low viscosity, and he has paraffins, which are less dense, as a family for a given boiling point.

Q. Do you mean given viscosity?

A. And, in general, for a given viscosity. So that by mixtures of these, as he says he has——

The Master: What do you mean by mixtures? Do you mean that he mixes one kind of oil with another kind of oil?

The Witness: Oh, no. I mean that he uses these oils and sometimes gets one kind of product and sometimes another kind of product. For example, on page 1488——

The Master: Mixture was not the word—you mean varying kinds of oils?

[fol. 1372] The Witness: Yes, varying kinds of oils, some of which were mixtures.

The Master: What do you mean by some of which were mixtures?

The Witness: I mean some of the oils were paraffins at the first ends, and aromatics at the last ends.

The Master: Was that inherent in the structure of the oil that he used?

The Witness: That was brought about by the cracking of the particular oils which he used.

Q. In any distillate that he got, did he have just a single hydrocarbon, or a number of them—a mixture of them?

A. You have always a number of them. For example, kerosene is made up of a number of hydrocarbons, but it is made largely of one family. Whereas, in his particular case, he was using products which were made up of more than one family—that is, aromatics, for example, along with the paraffins and olefins.

Mr. Wiles: Where is the citation of aromatics? I want to get this clear in my mind.

The Witness: Along in the middle of the—just above the middle of page 1488. "They are very rich in aromatic hydrocarbons."

The Master: What oils are very rich in hydrocarbons?

The Witness: The ones which Aisinman had been handling—the ones he produced.

The Master: Does he say what the origin of those oils is?

The Witness: Russian, I think he says, and I think, perhaps, Roumanian oils, as well. It is a German article.

Q. On page 2320, Defendants' expert states that:

"Aisinman's oils are the heavier products of an ordinary coking distillation without pressure,"

and on page 2321, he says:

"There is no reason, whatever, to think that Aisinman would get anything but a typical series of oils made from mid-continent, or from whatever type of oil he was working on."

What do you say in regard to this statement?

A. The fact that Aisinman, by his simple type of cracking operation, got large quantities of aromatics—which shows that he was not working on such oils as mid-continent oils, because with a simple type of cracking, we do not get large quantities of aromatics from ordinary mid-continent oils—we get, perhaps, small quantities, but very small quantities.

The Master: In his cracking operation, was he starting with the intermediate oils obtained from low distillation?

The Witness: He starts, in some cases, from crudes, but I don't recall, at this instant——

The Master: Everybody starts from the crude, but I mean in his cracking operation?

The Witness: In his cracking operation—I don't recall that he starts from an intermediate oil. I think that he has taken off his gasoline and kerosene, and then cracks the remaining portion. I am not certain but what he speaks, also, of the use of an intermediate, or gas oil.

The Master: At any rate, he was using some cut below kerosene and tar?

The Witness: Yes, sir. In fact, he was using all the cuts.

Mr. Wiles: So that what was left in the stills was a straight crude coking run.

Q. On Page 2321, speaking of the Aisenman product, defendants' expert stated that Aisenman speaks, on page 1489 of the book of exhibits, in the last three lines, of oil varying in specific gravity from .8 to .9 and in viscosity from 2 to 4 Engler, that of course that means to anyone skilled in the art that he had a series of oils varying from one oil with a viscosity of 2 and a specific gravity of .8 up to a viscosity of 4 and a specific gravity of .9. Would anyone skilled in the art give these lines the interpretation given by defendants' expert in view of the context of the exhibit?

A. No, a man familiar with the entire article should not take this view. As I called attention to, Aisenman refers particularly to aromatics in the article, these are products of high density and low viscosity.

[fol. 1373] Q. On page 2357. defendants' expert was asked:

"As a matter of fact was the knowledge of the industries sufficiently advanced, at that time, to make such a question as that at all probable?"

That is, to prepare a substantially gum free gasoline from unrefined cracked distillate, and that refers to 1913. The answer was, "No, it was not."

Was that answer correct?

A. That answer was not correct. The gum free gasoline was prepared and sold from cracked products as shown by——

The Master: He said uncracked.

The Witness: Gum free products are prepared from cracked products.

Mr. Wiles: That has not anything to do with the question.

The Master: If I heard the question, the question and answer of Mr. Wilson correctly, he referred to *one* cracked distillates.

Mr. Rauber: No, unrefined cracked distillates.

Mr. Wiles: What I put to him, and what I was getting at was this—that in 1913 nobody appreciated even the presence of gum well enough to ask any questions about it.

The Master: Let him answer the question.

Mr. Wiles: I do not care whether he made a gum free gasoline or not.

The Master: That appears on the record.

A. (Continued:) The knowledge was sufficient, and in fact such products were on the market as commercial products, in competition with gasoline made from crude petroleum by straight distillation. This is shown by the high bromine numbers given in Exhibit No. 113, page 1405, where shale naphtha is given, having a bromine number of 94, and that would correspond to an iodine number of, roughly, 160—155 or 160.

Q. Your statement that the bromine and iodine numbers stand approximately in the relation of 1 to 1.6 has been criticized. Is that criticism justified?

A. No, for we have, for example, in Exhibit 115, page 1417, a statement that the iodine absorption of hydrocarbons give results comparable with those obtained by bromine—this is not an exact quotation—if the former be multiplied by the factor .63. The .63 times the iodine number would give the bromine number, and would be the same as 1.6 times the bromine numebr to give the iodine number, one number being the reciprocal of the other.

Q. Who is the authority for that?

A. It is from Allen's Organic Commercial Analysis, of 1910, the particular article being by Dr. Garrett. Further, in confirming my view that it was as I said, roughly, 1.6, we have in the Hall patent No. 1239099—which I believe is an exhibit in this case, though I do not have the number of it [fol. 1374] as an exhibit. On page 3, line 15, we have for a cracked gasoline the bromine number, the iodine number and also the sulphuric acid absorption given for the same cracked gasoline. We have one called "A" and another one called "B." If we divide the iodine number two hundred and thirty-six by the bromine number one hundred and thirty-two, we get a factor of——

The Master: Do you mean a ratio?

The Witness: A ratio of 1.78, and if we divide the second one two hundred and fifty-six by one hundred and fifty, we get a ratio of 1.70, showing that these are, as I gave in my prima facie case for cracked gasoline, in roughly the ratio of 1.6. My figure is a little low, but it was given only as a rough figure. It is 1.7 and a fraction.

Mr. Cooper: Just to put it on the record, that patent to which reference is made, the Hall patent is Exhibit No. 511.

Q. Do you find any basis in the Ellis patent No. 1318060 for the assumption that the iodine numbers were taken according to the Hebal test, as indicated?

A. No, Ellis does not state what iodine method was used.

Q. There were a number of other methods in use at that time, were there, in 1913?

A. Yes, quite a large number. They all gave results that were used, though not closely agreeing results.

The Master: The iodine numbers are given in that patent?

The Witness: Yes, it speaks of the iodine number as being thirty or seventy or eighty, as the case may be; but does not give the method by which they are determined.

Q. On page 2356 defendants' expert stated that Ellis recommended the use of relatively small quantities of 40 to 60 per cent acid, followed if necessary by a small amount of slightly stronger acid, and that by this Ellis found it possible to make a fairly heavy gravity and relatively high end point gasoline, which normally carries a good bit of gum forming constituents and a bad color, into a satisfac-

tory gasoline with a loss of only one or two per cent and leaving in it more of the olefines, which it was previously considered had to be removed in order to make a satisfactory gasoline.

Is this statement correct?

Mr. Wiles: What exhibit number is that?

Mr. Rauber: Exhibit No. 175.

A. I find no statement regarding the use of a small amount of these acids. In fact, he does recommend in Exhibit No. 175 of this patent, page 1926 of this exhibit book, line 115, the use of stronger acids, "if required to remove olefines not extracted by the weaker acid."

The Master: Is that page 4 of the patent? I want to get it.

The Witness Page 4 of the patent, No. 1318061, line 115, and the stronger acid, in the same column, line 87, we have mentioned, as acid between 70 and 80 per cent of sulphuric anhydride. Eighty per cent of sulphuric anhydride would be stronger than 93 per cent; it would be almost 100 per cent sulphuric acid.

The Master: Am I correct in assuming that this Ellis patent refers to the purification of the cracked product and not to the cracking at all?

Mr. Rauber: I think that is correct, your Honor. That is the purifying process.

Q. On a page 2356, defendants' expert stated that in the prior art a gum free gasoline from a cracked distillate had been made by taking a very low end point product from the gasoline which, since the gum forming constituents tend to concentrate at the heavy end of the gasoline, can be largely eliminated by the simple expedient of making very light naphtha.

How does this expedient differ, if it differs from that described in the Ellis patent?

A. Ellis follows this same procedure and made cuts at 105, 125 and 150 degrees Centigrade, which would be at 221, 257 and 302 degrees Fahrenheit respectively. In other words, he suggests the use of a low cut.

Q. They would form low end point products, would they?

A. They would.

Q. On page 2360 defendants' expert, speaking of the treatment of cracked material with sulphuric acid, said that the natural assumption was that the acid would have to be increased from an amount sufficient to care for one or two [fol. 1375] per cent of olefines, as in straight run gasoline, to fifteen or twenty per cent, which represents the olefine content of the cracked gasoline, if you were to obtain an equally satisfactory product. Do you find any basis for such an assumption prior to 1913?

A. I do not find justification for such an assumption. The resinifying of the gum materials does not use up the acid, except in extremely small amounts. Only these reactions use up the acid where the sulphuric acid is chemically with the materials. This was known, and is well illustrated by the data I gave a few minutes ago from Allen's Commercial Organic Analysis, 1910, an article by Garrett, in which he shows that the products of commercial character were made without using sufficient sulphuric acid to use up the olefines, for these products carried quite large percentages of olefines, as shown by their bromine numbers; for example, a bromine number of 94.

Q. In that article does Allen describe the treatment of sulphuric acid, or just the product?

A. Just the product, which would go through the ordinary treatment of that day, which was known to be a sulphuric acid caustic soda treatment for that day and for the previous forty years or more.

The Master: You are now referring to this Hall patent still?

The Witness: No, to the Ellis patent still.

The Master: I mean the Ellis patent.

The Witness: Yes.

The Master: He is not concerned merely in using sulphuric acid for the treatment of cracked gasoline?

The Witness: Yes.

The Master: It is the manner of using it, substantially.

The Witness: It is his manner, but the point was we knew how to do or to accomplish this before this date. That was the question here.

Q. You mean by that you knew how to apply sulphuric acid in the treatment of a cracked product, or to purify gasoline?

The Witness: Both.

The Master: Have you pointed to any reference where this manner of using a weaker solution and then a stronger solution following has been employed?

The Witness: I did in my prima facie and I can give it here. I happen to have a note of it. We have the use in Exhibit No. 116. Redwood uses a weak acid on shale oil.

The Master: Yes.

The Witness: Mills in Exhibit No. 245 uses first a weak acid, an acid of approximately forty per cent—I think it is thirty-nine and a fraction per cent—and then a strong acid of ninety-two per cent.

The Master: Following it?

The Witness: Following it, which, of course, is exactly similar to the process described by Ellis.

Mr. Wiles: What was that used on, the same kind of oil or something else?

Q. What was it used on?

A. The process is the same, and we are discussing, I think, a process patent.

Q. What was the material acted upon in that process patent?

A. He was working on a shale illuminating oil; not a gasoline, but a shale illuminating oil of 1890. It must be borne in mind that the kerosene or illuminating oil of 1890 carried much of present day gasoline. So actually he was working on a product parallel to present-day gasoline in part.

The Master: But he was working on kerosene.

The Witness: The then kerosene which is not our present kerosene.

The Master: I understand.

The Witness: But the then kerosene carried a fair proportion of gasoline.

Q. Was the process also applied to gasoline, shale gasoline?

A. I think so, though I do not find it here.

Q. Do you consider a shale kerosene or shale gasoline a cracked product?

A. They are cracked products.

[fol. 1376] Q. On page 2367 defendants' expert stated that none of exhibits 113, 116, 117, 118, 119, 158 and 245 dis-

closes treatment with acid in the range or from 40 to 60 per cent. Is that statement correct?

A. No. For example, the Mills citations which I have just given, exhibit No. 245, page 4199, discloses acids of approximately 40 per cent—that is, 39 per cent. Letny, Exhibit No. 158, page 1876, in the last paragraph, discloses the use of approximately fifty per cent of sulphuric acid. Redwood, Exhibit No. 116, discloses the use of sixty degree acid, which is a little more than sixty per cent; it is in the 70's. Vittenet, Exhibit No. 117, also uses sixty degree acid.

Armstrong & Miller used first 67 per cent sulphuric acid and then 80 per cent. This is exhibit 119, and is given on page 1508, the 67 per cent as two volumes acid to one volume of water, and then later as the 80 per cent four volumes to one.

So that we have in these exhibits acids in the general range of from forty to sixty per cent, used for the treatment of petroleum products.

Q. On page 2386, defendants' expert stated that statements that benzol is an unsaturated hydrocarbon were not precisely correct. What do you say in regard to this statement?

A. I will first quote a late book of 1924, in German, by Holde, which I have chosen because it is the only book which defendants' expert cited, and apparently one which he considers as an authority. This is a discussion of the hydrocarbon materials from the technical side as well as the purely chemistry side, as shown by the title page. I will translate as I read, page 112, line 10:

"The aromatics and also the other unsaturated hydrocarbons."

He is here discussing methods of treating petroleum oils and considered aromatics as unsaturated hydrocarbons. Going to the older texts but standard texts, we have in Pinner's Introduction to the Study of Organic Chemistry, page 13, "The hydrocarbons which are capable of taking up more hydrogen are termed unsaturated."

The Master: That is just exactly what Mr. Wilson said.

The Witness: Yes. Then on page 177 we begin a chapter on unsaturated hydrocarbons, and on page 179, as the third division in that chapter, the aromatic series is described.

The Master: And benzol is included there?

The Witness: Yes. Benzol is a typical aromatic hydrocarbon. Probably the world's greatest authority in the earlier years, on petroleum, was Engler. We have had, probably, in the world's history only one other man who could compete with him for general knowledge of the petroleum industry, and that was Redwood.

"In "Exhibit No. 248," page 4240, the statement is made, in line 7.

"Whether the high pressure distillate consists principally of unsaturated (Cn. Hn. aromatic) hydrocarbons, three principal factions of the same were shaken successively."

Engler, in other words, counts aromatic hydrocarbons as unsaturated.

As particularly bearing upon this, we have, in the Ellis File Wrapper, of the original application, a statement by the Examiner—on page 2754, "Exhibit No. 209." The Examiner states:

"It may be said that a mixture of pure gasoline and of benzol would anticipage the third, fourth and fifth claims."

These are claims given on page 2758.

Claim three, in which the product "has an iodine number of at least thirty"; claim four, a product "containing a substantial amount of unsaturated bodies"; claim five, "containing a substantial amount of unsaturated bodies."

I have not read all the claims, but the pertinent portions, only.

The Examiner considered that those claims would be anticipated by benzol. Ellis apparently took his view also, because, at once——

The Master: You mean he acquiesced in it?

The Witness: Yes, sir, he acquiesced in it, directly and not through his attorney, by canceling claims three, four and [fol. 1377] five. This is the application from which, later, the product patent was made, either as a continuation, or as a division. In other words, Ellis considered benzol as an unsaturated compound.

Mr. Wiles: He recanted very shortly, from that acquiescence.

The Witness: I will call your attention to the fact that this was not due to an attorney, but was signed by Ellis, himself. These citations show that aromatics are commonly considered in the petroleum industry, as unsaturates.

Q. On page 2387, defendant's expert said that you could not apply a sulphuric acid absorption test to benzol, if you shook the benzol and sulphuric acid while immersed in a mixture of ice and water, because the benzol would freeze and the acid would then merely be swished about over the top of the frozen benzol.

Is this statement true and correct?

A. The benzol which I used in my tests, which were carried out a year ago, was the so-called pure benzol, boiling at 80 to 82 degrees Centigrade, and it is to be noted that contrary to the defendant's expert's statement, this does not freeze at 42 degrees F., but at some lower temperature. In other words, it does not freeze at the temperature he said.

Now, it must be further borne in mind that using such a benzol, and following the ordinary care and method used in making sulphuric acid tests on petroleum products, we will not have the freezing of the benzol, contrary to the statement that has been made. I have tried it, and is does not.

Theoretically, this might be true, but the fact is, as soon as the sulphuric acid has acted on the benzol, to form benzene, or sulphonic acid, that this new material dissolves in the benzol and lowers the freezing point, so that it does not freeze—so that the benzol does continue to react.

The Master: You mean lowers the freezing point?

The Witness: Yes, and keeps it liquid, so that it does not freeze. Instead of freezing—

The Master: Do you mean raising the freezing point?

The Witness: No, your Honor. Instead of freezing at 42, it might freeze at 28 or 30 or 25, but not at the temperature at which it is acting.

Other aromatic compounds act even more readily and completely—such as toluene, xylene, and simliar products. Toluene was made from petroleum as a war product, during the war, for explosive purposes.

[fol. 1378] Q. On page 2392, defendant's expert stated that he was unable to find a paper by you, saying that certain saturated hydrocarbons were completely soluble in sulphuric acid. Will you state the name and date of this publication?

A. I forgot to look it up exactly, but I can give it roughly, so that it may be located without difficulty. It was in the magazine "Science", of about 1912, or 1913.

Q. On page 2394, defendant's expert quoted you as saying, in an article in "Canadian Chemistry and Mettal-urgy", for January, 1923, in speaking of Scotch shale gasoline, that they are strongly odored and somewhat colored, and cannot be stored for any considerable time, without loss in quality.

Are Scotch shale gasolines any different in this respect from modern cracked gasolines?

A. No, they are substantially the same.

Q. On page 2396, defendant's expert criticized your statement that in reclaiming spent sulphuric acid, in the petroleum industry, the cost of producing sulphuric acid of 75 per cent. strength is practically the same as making ninety-three per cent., or 66 degree Baume acid, because the acid must first be concentrated to the higher strength, and then diluted back."

What is the fact in regard to concentrating spent acid?

A. The acid is used on the petroleum hydrocarbons, and then, in order to bring about a separation of certain hydrocarbons, sludge tar materials, it is diluted to about a 35 degree Baume acid. This would be somewhat higher in percentages,—perhaps 45 per cent. acid. It is allowed to settle and the tar like materials withdrawn. The acid will carry a considerable amount of hydrocarbons in it, as chemically combined compounds, with the sulphuric acid. The amount will vary from one to—well, with some lubricating oils, up as high at 8 per cent. hydrocarbon calculated as carbon. That will make it, perhaps, nine per cent. hydrocarbon. In many cases it will carry three per cent., calculated on the acid, or one and a fraction per cent., calculated on the total volume.

On evaporating this, in order to concentrate it in the modern type of concentrator, such as is used by many plants, both in this section and in other parts of the country—particularly the type of plant that I mentioned in my prima facie case—in my testimony of a year ago—the concentrator made by The Chemical Construction Company. We have to evaporate this to a fairly high concentration before we can decompose these organic compounds. They must be decomposed before the acid is fit for use in treating hydrocarbon products—petroleum products—and it is necessary to concentrate to approximately 64 degrees Baume, which would be 85.7 per cent. sulphuric acid, before we can get these compounds to act with sulphuric acid, and be decomposed by the sulphuric acid.

In order to insure this, it is rather customary to go to a slightly higher percentage than this, say to around 90 per cent.

It will not do to concentrate to 75 per cent. acid and then attempt to use this acid on petroleum products, such as gasoline, or kerosene. It will not react satisfactorily, and will not serve.

Nor will it even serve to concentrate to 75 per cent. and then add fuming sulphuric acid to bring it up to the desired per cent.—say 80, or 85 per cent.—because the presence of these hydrocarbon materials prevent the satisfactory use of this sulphuric acid, for the treating of kerosene, or gasoline.

From this we come, then, to the fact that when we want a 75 per cent. sulphuric acid from a recovered acid, it is necessary to concentrate to 85 to 90 per cent., and then to dilute that to 75 per cent. acid for our use.

Or, in other words, my statement, which I made without giving the reason for it at the time, that it is as cheap to concentrate the acid to 66 degrees—which, it happens, is 93 per cent.—as it is to recover for re-use commercially at 75 per cent. is correct.

That is, in order to use sulphuric acid for petroleum work, we must remove these hydrocarbons, which are dissolved in the waste acid, and to do that we must concentrate to, in the range of 90 per cent.—64 to 66 degree acid.

Q. Is that process that you have been describing, a commercial process?

A. That is the present day modern commercial process.

The Master: For recovering the sulphuric acid?

The Witness: Whether to be used as 75 per cent., or as 90, or 93 per cent.

[fol. 1379] Q. On page 2402 defendant's expert stated that the term, "cylinder oils," as used in the references, always means a residuary product, which has never been distilled.

Is this statement correct?

A. No. As I stated in my testimony of a year ago, cylinder oils are customarily made from residual products, but that they are sometimes made from distilled products. And that is the truth. They are not always made from residuum products.

Q. On page 2500, defendant's expert stated that the gas oil described by Burton as being made from petroleum that was capable of making lubricants and wax.

Can lubricants and wax be made from Scotch shale oils?

A. They are made commercially, in large quantities.

Q. Burton, in his patent No. 1,049,667, describes certain ethylene products as being objectionable. What are ethylene products as a class?

A. Olefins—that is, they are the unsaturated hydrocarbons belonging to the olefin class.

Q. It is stated that a column still of the Pielsticker apparatus, "Exhibit Nos. 133 and 134," is the type used in the benzol industry. Would its function as a fractionating tower, or bubble tower, be ordinarily understood from this description?

A. Certainly. Anyone familiar with chemical industry would understand it.

Q. How do column stills, such as those used in the benzol industry, compare with the bubble tower, such as shown in Lewis and Cook's patent No. 1,392,584, and as shown in Barbet patent, "Exhibit No. 156,"?

A. They are substantially the same.

Q. As the benzol column still?

A. They are substantially the same, yes, as is used in the benzol industry.

Q. Would a fractionating column, or column still, of the type used in the benzol industry, normally be used purely to stop entrainment?

A. Not normally. One would not use an expensive piece of apparatus when some inexpensive apparatus would serve equally well.

Q. What do you find to be the purpose of the column still, in Pielsticker's patent, "Exhibits Nos. 133 and 134"?

A. I would think that it was used for the ordinary purpose for which a column still is used—that is, as a fractionating column, or to permit the close separation of the products which it passes over, and the product which it is wished to be taken off at the bottom of the column.

In this connection, in "Exhibit No. 604," the whole subject of column stills, in this short article by Professor Thorp, of the Massachusetts Institute of Technology, is well discussed, and it shows the use of column stills in various industries.

[fol. 1380] Q. Defendant's expert has contended that the heaviest kerosenes made in the Barbet process, "Exhibit Nos. 136 and 141", is a gas oil. Is this true?

A. No. Barbet describes these products as illuminating oils—the heavy being the ordinary illuminating oil.

If we would look on page 1721, of "Exhibit No. 138," where Barbet is discussing an apparatus for the continuous distillation and purification of petroleum, he describes in lines 5 to 7, the three paraffins, or illuminating oils which he makes. These lines read:

"Numbers 7 and 8, respectively, of about 0.780, and 0.800 condensates are petrols deluxe. No. 9 is a more common petrol. It still contains a little heavy oil."

Just above he has stated that the products, in gauge glass, No. 6 has a specific gravity of 0.769. In other words, if we transfer these into Baume, the lightest would be 52.5, the medium 49.9, and that having the greatest density given, 0.800—45.4 degrees Baume.

If we would take the change in gravity, between the successive fractions, and continue it on for the next fraction, which presumably will have a similar drop in gravity, we would have a gravity of between 41 and 42—or about 42—possibly a little over 42—which is the same as our ordinary good grade of kerosene of today, and has been for quite a while.

The Master: That is not describing the patent. You are assuming that the ratio between the cuts——

The Witness: That the gravity would drop.

The Master: That is; the ratio of gravity between the cuts is constant?

The Witness: Is approximately constant.

The Master: That is a large assumption, isn't it?

The Witness: No, it is not, because on the previous page, your Honor, we have produced from the gauge glasses, 1, 2, 3, 4, and 5, which are dropping about 5 degrees Baumé, successively.

And the same thing is true of these. But the drop is getting slightly less, as you go—that is, it is first around—the first one is actually ten degrees, then 6, then 4½, then about 5, then 3, and then about 4. So that if we would take from 45.4, subtract 4, we would have 41.4, which would be con-

tinuing the same drop in gravity, which the other fractions [fol. 1381] have had, and that would not be one carrying any appreciable amounts of heavy gas oil—or of gas oil. Gas oil has a gravity of 34 or 35—or something like that.

Q. Do the Baume gravities decrease steadily as you go up in boiling point range, in the distillation of a crude?

A. Yes, but tending to make less and less differences. That is, they continually decrease, but not so rapidly as they do at first.

Q. There is nothing to indicate that they would drop rapidly after you got below 45?

A. Oh, no.

Q. On page 2576, defendant's expert stated that you can call gas oil and paraffin distillate, kerosene. Was that customary, to call paraffin distillate, kerosene?

A. No, nobody with a knowledge of the petroleum industry would call paraffin distillate that.

Q. Calling your attention to the dephlegmator of the Schuchlow, et al., patent, "Exhibit No. 129," what would be the usual, or normal location of the dephlegmator for dephlegmating, or fractionating the vapor separated in the chamber, A.

A. At the top of chamber A.

Q. If cold feed were used in this dephlegmator, where would this feed enter?

A. Near the top—probably at the small dome indicated in the drawing as the top part of chamber A.

The Master: Was any such thing shown on the Schuchow and Gavrilow patent?

Mr. Rauber: He said, "or a dephlegmator with cold feed." Of course, what I want to bring out is where a person would normally put that cold feed, in dephlegmating.

Q. Could he put the cold feed into the bottom and dephlegmate?

A. No.

Q. On page 2610, defendant's expert stated that a good chemical engineer would not leave aside the subject of entrainment, in designing a bubble tower, or fractionating column.

What bearing would this matter of entrainment have on the question of whether the reflux material entered on the top plate of the column, or one or two plates below the top?

A. In order to avoid entrainment, and for other reasons, it is customary to have the returned reflux condenser enter at one or two or three plates below the top.

[fol. 1382] In confirmation of this I wish to refer to the "Elements of Fractional Distillation," by Robinson, 1922, which is one of the more commonly used books on fractional distillation, Robinson being a professor at the Massachusetts Institute of Technology.

In this I wish to call attention to the fact that in the preface, a few lines from the last, he thanks Professor Lewis, of the Institute, for help that he has given him in this matter.

On page 116 we have these dephlegmators entering at plates here, on the third plate, rather than at the top—or, on the second, if that top line is not a plate. We have the same thing again similarly in the right hand column still. We have, similarly, on page 117, the dephlegmator reflux material entering on plates below the top plate.

On page 126 we have two other column stills in which—in fact, we have three—in which it enters not at the top plate.

On page 146 we have a discussion of the advantage of continuous stills over intermittent stills, and the statement is made, at the bottom of the page, in the last paragraph, under the heading of "continuous stills."

The use of a continuous rectifying still, as developed by Barbet, has enabled alcohol producers to put on the market an alcohol of higher grade than the best produced by the intermittent still, without subsequent chemical treatment with charcoal, and so forth."

On page 147 we have such a continuous still of Barbet, illustrated—and this, I may add, is the same Barbet as we have cited several times in this case. And it is to be noted that the reflux returning from the dephlegmators in each case, enters onto plates well below the top of the column still.

The Master: All of that refers to the art of alcohol distillation, does it not?

The Witness: No. This is a general discussion, of which only the latter part refers to alcohol. The book is not on alcohol distillation.

The Master: But the discussion is all about the art of distilling alcohol?

The Witness: No, your Honor. The discussion is regarding other things, but as the defendant's expert has brought out, the development of stills, and column stills particularly has been one which has been developed in the alcohol industry, and then carried over into other industries.

But this book is not concerned, except in a minor way, [fol. 1383] with the alcohol industry, but has used it in some places, as illustrative material, only.

The Master: What is the date of that book?

The Witness: 1922.

Q. On pages 2611 and 2612 Defendant's expert stated that dephlegmation of vapors and stripping of vapors are not the same thing. Is that statement correct?

A. The process is the same and is commonly called by either of the two names.

Q. Defendant's expert stated on page 2614 that the usual type of dephlegmator, as the term is used in text books does not include the step of stripping. Is that statement correct?

A. Fractional condensation—that is, condensation of one portion and not of the lighter portions—is a stripping process. In Exhibit No. 604, page 9 of the Thorp photostat, at the middle of the page, we read:

"In practical work the appliance for fractional condensation is placed between the scale and the condenser. It may be an apparatus, called a 'Dephlegmator,' in which the vapors are forced to bubble through a layer or column of the condenser higher-boiling liquid as in a tower; or the liquid vapors may pass through chambers or a pipe, kept at a constant temperature, just above the boiling-point of the low-boiling liquid."

The Master: Let us not get into a controversy here about terms. The term "Stripping" for instance. It is not whether you call it stripping or melting, or whatever you may call it to describe something that might take place. The question is whether the things that take place are the same things.

The Witness: Stripping and dephlegmation are substantially the same thing.

The Master: Well, that is a question, whether they are or not. I remember when I first heard that word "stripping," I think it was used by Mr. Wilson in connection with Humphrey's run-back. If what occurred in Humphrey's run-back is the same thing that occurs in the dephlegmator or in the bubble tower, why then the stripping may be applied to the two. But stripping may be a misleading term, to apply to what took place. At any rate, I think the essential thing to keep your mind on is, what is it that is taking place, not what you call it.

The Witness: And yet we have to use words to express it.

Q. Do the same things take place in the Humphrey's run-back and in the ordinary dephlegmator?

[fol. 1384] A. Yes, it is substantially the same process.

The Master: Well, all of the same things?

The Witness: How is that?

The Master: All of the same things, or some of the same things?

The Witness: They differ only in degree. All of the things take place, but there may be differences in amount, according to how the dephlegmator or the run-back is made, what its length is, and so on, how much cooling takes place.

Q. On page 2633 of the printed record Defendant's expert stated that up to 1910 the term "intermediate oils," had no definite meaning apart from the context. Do you find any commonly accepted meaning for this term as applied to oil products prior to 1910?

A. The term "intermediate oils," is used more in England than it is in this country, and is referred primarily to the distillate oils of what we commonly call gas oil range. This is illustrated, perhaps, by the methods of testing used in England, and which correspond to our official methods of testing used in this country, as worked out by the American Society for the Testing of Materials, in which what we call gas oil is called by them "intermediate oil." And they have also certain other fractions near this fraction which are spoken of as intermediate oil.

Q. Was that true prior to 1910, in regard to intermediate oils?

A. Yes.

Q. On pages 2668 and 2669 of the printed record Defendant's Expert stated that unless a temperature were given in a statement of Engler viscosity he would not know which temperature was meant. Is there a standard temperature that has always been meant when no particular temperature is specifically mentioned in connection with Engler viscosities?

A. Yes, 20°C is the standard temperature for determining the viscosity by means of the Engler apparatus. In confirmation of that I wish to quote from a book on oil analysis by Professor Gill of the Massachusetts Institute of Technology, Sixth Edition, page 26, in which he states——

Mr. Wiles: What is the date?

The Witness: 1911.

Mr. Wiles: Objected to as immaterial.

A. (continued). Page 26, line 13:

"The instrument" referring to the Engler instrument—"is standardized with water, 200cc of which at 20°C should run out in 50 to 52 seconds."

And then in describing its use with oil, the oil "is poured into it up to the level of the studs, stored until 20°C is reached, and both adjusted to the same temperature."

This which is present in this edition is, I think, also present in the earlier editions as well. It is standard procedure, as given here by this authority from the Massachusetts Institute of Technology, and it has been the customary practice and is at present the customary temperature of operation.

Q. On page 2676, Defendant's expert stated that in ordinary straight-run kerosene you would not have five per cent of olefins. Is this statement true?

A. This statement is not true. On page 1422 of Exhibit No. 115 we have the statement that the kerosene or burning oil from American petroleum contains "50 to 80 per cent of higher members of the methane series, the remainder chiefly olefins."

So that we would have, then, that of the 20 to 50 per cent after removal of the methane series, would be largely olefins.

On page 1424 of Exhibit No. 115—and these are at a date when it was not customary to make kerosene by cracking American petroleums——

Mr. Wiles: What is the date, please?

The Witness: The date of the publication is 1910, the work being carried out previously.

A. (continuing.) We have the burning oils, which would be kerosene, from American petroleum, having a bromine number of 34.8. If we use the Hall patent to interpret this——

Q. Which Hall patent?

A. The Hall patent, Exhibit No. 511, page 8, line 15, it would bring us to a percentage of unsaturates—that is, olefins, as used here—of upwards of 17 per cent, which is of course considerably more than the five per cent. Fur- [fol. 1385] thermore, Petitioner's Exhibit 113, page 1405, of day 1881, an article by Allan calls attention to the fact that from petroleum we would have a bromine number of 17.2 which, using the Hall patent, Defendant's Exhibit No. 511 to make the comparison with, we would have that the percentages of olefins would run upwards of 8.5 per cent.——

The Master: In kerosene?

The Witness: In kerosene.

Q. If you treated such a product having these quantities of olefins with one or two per cent of sulphuric acid, would you remove all of the olefins?

A. No, you would remove only part.

Q. On page 2682, defendants' expert has pointed out that the refined saturated paraffin wax gave a bromine value of 5.6 and an iodine value of 1.11. How do you explain this phenomenon?

A. In my testimony of a year ago I called attention to the fact that the figures for bromine value and iodine value were only approximate; and, indeed, we may take them as true only within two or three whole units normally. So that these figures agree almost within the limit of error. However, bromine is a more reactive chemical than iodine, and I would expect that it might have in general a more

positive error—that is, that the number would run a little larger for the bromine number than it theoretically should, due to its being a somewhat more reactive chemical. Iodine might even run below the theoretical amount, being a slightly reactive chemical. However, the error due to this is always slight, and in practical use it does not amount to anything.

For example, on page 3, bromine numbers are given as 150——

The Master: Page 3 of what?

The Witness: Of Exhibit No. 511, the Hall Patent No. 1239099.

A. (continuing:) The bromine numbers of gasoline are given as in the range of 150 and the iodine numbers 256. So that a small error here, an error of two or three would be a very small percentage of the total iodine number or bromine number. It is not appreciable in its effect in ordinary use.

Q. On page 2683 defendants' expert stated that it frequently happened that the iodine number is smaller than the bromine number. Is this statement correct?

A. I do not know of any case in which this is true, ex- [fol. 1386] cepting where the iodine and bromine numbers are very small—that is, of the order of one to five or one to six. There may exist numbers a little larger, but not numbers such as the iodine numbers ordinarily run to, which may be in the order of 100 to 300.

Q. On page 2687 defendants' expert in speaking of distillation stated; stated there are two ordinary places that a thermometer is placed in ordinary work, one is in the liquid, one is in the vapor.

How would readings of the two thermometers thus placed compare relative to each other?

A. In distilling liquids such as we are concerned with here, such as gasoline or kerosene, that in the liquid would be much above, that is, of much higher temperature, than that in the vapor.

Q. In making a distillation test of gasoline or kerosene, where is the standard location for the t-ermometer bulb in the distillation test?

A. In the vapor.

Q. On pages 2687 and 2688 defendants' expert stated that if you cut a distillation at 105 or 125 or 150 degrees C, you would not necessarily have a suitable gasoline. How does this cut compare with those described in the Ellis patent No. 1318060 as a suitable method of making gasoline by his process?

A. These are exactly the temperatures of his cuts, as disclosed by Ellis.

Q. What is the evidence, if any, to show that there is no essential difference between shale oil, particularly the European shale oils, and well petroleum?

A. In general, your Honor, I may say that the content of European shale oils, so far as their constituents are concerned, are in general intermediate between known sources of well petroleum.

First, for example, the gasoline content is less than in Pennsylvania oil; but it is more than in many other petroleums, such as a Mexican or many American petroleums.

Second, the nitrogen content—that is the content of nitrogen compounds, such a pyridine and quinoline bases—is intermediate between well petroleums. We have more of them in California petroleum by several times, though the percentage present in, we will say, Scotch or French scale oils, is more than in some American petroleums. In other words, the nitrogen content in shale oil is intermediate as [fol. 1387] between various well petroleums. We do not have well petroleums, however, having approximately the same amounts of nitrogen compounds.

Third, the amount of the hydrocarbons of the saturated series are intermediate in shale petroleums, as compared with the crude well petroleums. It is less, for example, than in the Pennsylvania petroleum, but it is greater than in Russian and some Gulf Coast petroleums.

Fourth, the content of unsaturates of the shale oil is intermediate between petroleums. It is more than for most mid-continent petroleums, but less than some of the mid-continent petroleums and Texas petroleums.

Fifth, the content of sulphur in such European shale oils is less than in many Mexican and Californian petroleums, but greater than in Pennsylvania petroleums and in some mid-continent petroleums.

Sixth, the content of asphalt in the scale oils varies, but is intermediate between well petroleums. It is less than in

many Texas well petroleums or Californian well petroleums, but greater than most of the petroleums of the Appalachian field, and agrees with the asphalt content of many mid-continent well petroleums.

Seventh, the content of nap-thenes is less than in the Russian and Californian in many Texas petroleums, but is about as large as that in many of the petroleums of the Appalachian field.

Eighth, the content of aromatic hydrocarbons is lower, but in this it agrees with most well petroleums. We have only been able to prove the presence of aromatic hydrocarbons in extremely few well petroleums, but in some of them we have a considerable amount, such as that in Burma, East Burma particularly, and certain small amounts are found in certain California petroleums.

Ninth, I have not been able to find any constituents present in shale oil which is not found in less amounts—I am referring particularly to the Scotch and French shale oils—in some well petroleums, and in larger amounts in other well petroleums, and in approximately the same amounts in still other well petroleums. I know of no constituent of shale oil, even to the small extent of one per cent, of which this is not found to be true.

Tenth, further, I know of no constituent, even to the same small extent, found in well petroleums, which is not present in French and Scotch shale oils.

[fol. 1388] Eleventh, no chemist, I think, who was given a sample of Scotch or French shale oils would be able to say definitely that it was not a well petroleum and give satisfactory reasons for it. Moreover, the reverse would also be true for many mid-continent well petroleums: He would not be able to say that it was not a shale oil. There are some which we would be able to identify positively, for example, the Borneo well petroleum, as I know of no shale oil carrying as much in the way of aromatics.

There are some well petroleums which are at the other extreme, which carry such a large percentage of paraffins, larger than I know in any shale oil, but if given many a mid-continent or Texas oils, the oil expert would not be able to say determine by analysis whether it was a well petroleum, or a shale oil.

As data bearing upon the shale oils, particularly, there is an article by Conaches, who is I think Assistant District

Manager of the Scotch Shale Oils, Ltd., in the Geological Magazine of January to December, 1917——

Mr. Wiles: Objected to as hearsay.

A. (continuing): In which he gives the shale oil as containing paraffins, olefins, naphthenes.

Mr. Wiles: That is objected to.

The Master: It may be hearsay, but I am in some doubt about it. If there is a recognized work that class of hearsay in such matters is receivable.

Mr. Wiles: There is an immense amount mis-information in this industry; that is the trouble with it.

The Master: I realize there may be.

Mr. Rauber: If I remember correctly, the defendants themselves offered Government publications to prove the same things, or things of the same nature.

Mr. Wiles: Well, it was not objected to. I would be perfectly willing to call the man who made it, if necessary.

The Master: I doubt if we will have the time to call any Scotchmen. I will overrule the objection.

A. (Continuing:) Volume 12, of 1917, an article by Dean Winchester, who is here describing work carried out under the U. S. Geological Survey, gives for certain American scale oils asphalt content of two per cent, sulphur content .6 per cent and nitrogen content 1.7 per cent.

I should add before closing the answer to this question that of course we will have other shale oils which differ from the Scotch and French scale oils and which will vary [fol. 1389] among themselves, perhaps, as much as the well petroleums vary. But in general they are of the same kind of oils and have the same types of variation as the well petroleums do. We have not tested all well petroleums; we have not tested, of course, all shales, and the oils resulting from them. But in general the shale oils and well petroleums are substantially the same.

Q. In Redwood's Petroleum, certain pages of which have been cited or admitted in evidence as Exhibit 107, do you find any evidence to show that the Dewar & Redwood process was for the cracking of oils from well petroleum rather than from shale oil?

A. The citations from Redwood's "Petroleum" which are in as exhibits speak of it as a process operating upon

petroleum; and, as evidence that Redwood in this volume considered shale oil and petroleum not as synonymous words, but spoke of them separately, though with full knowledge of their similarity, we have the table of contents showing that Section 7 is entitled, "The Shale Oil and Allied Industries," whereas the section immediately following is entitled, "The Transport, Storage and Distribution of Petroleum," and the section immediately preceding it, "the Refining of Petroleum," and the section immediately preceding that, "The Production of Petroleum, Natural Gas and Ozokerite"; and following that Section 9, which is one of the latter chapters, is entitled, "The Testing of Crude Petroleum, Petroleum and Shale Oil Products, Ozorkerite and Asphalt."

The Master: You would say, then, that the terms "hydrocarbon oil" would include shale oils?

The Witness: Oh, yes.

The Master: And that the term "Petroleum" would include shale oils?

The Witness: I would say that the products are identical, and that early "petroleum" was used exclusively for a shale oil product. Later the term was taken over by the well petroleum people. But at this stage Redwood was distinguishing between the two kinds of petroleum, well petroleum and shale oil, so that the citation in this book is a clear one for well petroleum and is not for shale oil, as the defense's expert witness has suggested.

The Master: "Petroleum" by its etimology, referring to a rock oil, would in your judgment refer to shale rock oil as well as a well oil?

The Witness: Originally it meant only a shale rock oil. A hundred years before we had the first well petroleum it was used—the word "petroleum" was used. And then it continued on down, and is used still in a loose sense as covering the two. The two are identical, though.

Q. In this book of Redwood's did he treat the well petroleum as a separate chapter from the shale products?

A. Yes.

[fol. 1390] Q. On page 2764, Dr. Lewis' testimony, he was asked the question, "The flash point would be fairly close to the boiling point; is that statement correct? A. No,

that statement is not correct. The flash point of a kerosene is roughly 200 degrees Fahrenheit below the boiling point."

Have you any evidence to show that some kerosenes have a flash point that is less than 200 degrees Fahrenheit below the boiling point?

A. I have. In Exhibit No. 511, the Hall patent No. 1239099, on page 4, there is a product described whose distillation is given, which is a kerosene. This is shown by its specific gravity, its flash point, and the temperature at which it boils, giving the percentage, "which boils between each ten degrees Centigrade." The flash point is given at 94 degrees. Attention has been called earlier in this case to the fact that the burning point of an oil is approximately twenty to thirty degrees above its flash point; so that we would have its burning point approximately 120 degrees F. The boiling point began at 110 degrees C. 110 degrees C is 230 degrees F, and 230 degrees F is only 110 degrees above 120 degrees. In other words, very decidedly lower than 220 degrees by defendants' witness.

Mr. Wiles: Lewis was talking about flash point, not boiling point.

The Witness: But my testimony was in connection with the burning point.

Mr. Wiles: Oh, no, it was not.

Q. Dr. McKee, will you please trace the course of the pressure tar from the cracking still of Barbet patent exhibit No. 136, and of any distillates taken therefrom—any of the distillates heavier than kerosene—using for this purpose the defendants' enlargement of the drawings of the Barbet patent.

A. (Referring to drawing.) We have the pressure still P with the agitator Y, in which the cracking is taking place; the pressure tar is drawn off at Y, and passes up through the pipe and enters the still Q at the point marked Z. It flashes into vapor, and these vapors pass upward. A portion of the pressure tar passes down as liquid through these plates and drawoff parts indicated by the bottom line, the bottom of column Q, and there it comes into contact with the vapors from P, which are passed out through the line Z and enter at about the point W. These vapors remove addi-

tional volatile materials which might be present, so that non-volatile portions can be drawn at X, to be used as a fuel oil.

This defendants' exhibit is marked here, "to the paraffin presses." The exhibit itself mentions it to be used for burning in the furnaces, but also mentions that it might be used for paraffin.

Mr. Wiles: What it says actually is that it goes to the paraffin presses, or may be used as fuel, doesn't it?

The Witness: No.

Mr. Rauber: Would you mind letting the witness testify, and then you can cross-examine him.

A. I may add that there are a good many places of this where interpretations have been put by means of the lettered portions which are not merited in the diagram. There are, of course, none of these legends in the original drawing in the patent Exhibit No. 136.

The vapors pass through the tower Q, passing upwards and a part of them are condensed in the dephlegmator R, and circle back into the tower. These pass over through the pipe T, T', and consists of the gasoline vapors, the illuminating vapors, gas oil vapors, and possibly heavier products. It is marked here as if it were only illuminating vapors, but any gasoline vapors must go in this way, as well as illuminating vapors and gas oil.

The Master: But everything that is in R and passes out of R, is in the vapor phase?

The Witness: A portion is passed back through U in the liquid phase, but a portion of it is not. The portion passing back through here (indicating), by the way, is very high boiling, as shown by the fact that this R must be kept hot, with hot water at 70° C, which would be 148° Fahrenheit.

[fol. 1391] These vapors come into the tower, H.H', and are mixed with the hot crude oil, which has come down through the pipe B, to the heater G, and enters in at a point just below T', and we have then the passing down of the heavy portions, liquid portions, the passing up of the vapor portions in this tower H,H'.

The vapor portions, a part of them, pass out through the top of the tower and go to the gasoline purification system, which is not shown on this exhibit, but which is shown in Exhibit No. 136. The kerosene portions pass over from the plates into the condensers, coolers, O.O′ and O², and are collected at the points marked 6, 7 and 8.

Again I would call attention that the legends here are not such as the exhibit itself merits. There is no indication in the exhibit that one is heavy and one is light.

The Master: What would you infer, then, from—

The Witness: They are undoubtedly heavier, but that does not mean that it is heavier than ordinary kerosene.

Then they pass down through the lower plates of H, and it is to be noted that the patent, Exhibit No. 136, mentions that the plates H, are kept hot enough—page 1691, Exhibit No. 136. There it is mentioned that the illuminating oil vapors are evolved on the plates H. We are speaking here of H in contrast with H′. So that we have here the coming up of illuminating vapors from these plates, and by inference, then, the heavier vapors are condensed as liquid and pass down.

We do not know that that takes place, because we have the passing down here, shown by the dotted lines across there, and then to the bottom of M from the plates in which is the thermometer T⁵, we have a pipe indicated, passing to the bottom of the still M.

The Master: Those vertical lines, light lines, are pipes?

The Witness: Yes.

The Master: So described in the patent?

The Witness: That is customary for a bubble tower arrangement,—that is, you have here on one side a down pipe, and the next one a down pipe on the other side, similarly, which would lead us to the conclusion that in the bottom plate here this is running to the bottom. No mention is made of them, though, that is obvious, I think, in general.

These heavy liquids, gas oil and heavier materials, passing down here, then would in part be passed out through the pipe at the bottom of the diagram, unmarked, to the pump X, and then be pumped back to the pressure still P.

[fol. 1392] The Master: What is the tank M charged with?

The Witness: Tank M is charged with two things; It is

charged with the crude which comes down in the pipe marked to the left of P into the tank G, and out upon the enlarged middle plate of the tower H,H' and then down through the down pipes of the several plates to the bottom M. That is one material. That material is crude petroleum which has had its gasoline removed before reaching this part of the column, and which has had its kerosene removed on the plates of H.

The Master: Yes, but it is not crude when it reaches M, is it?

The Witness: It is a reduced crude, that is, a crude lacking the gasoline and kerosene. The other portion that gets into M, is the part of the pressure tar that passes out from Y through the tower Q, and through the dephlegmator R and the pipe T,T' into the tower H,H', and then down in the same way into the tank M.

The Master: The open end of the last pipe, opening into M and going near to the bottom of M, is lighter than the reduced crude with which M is charged, isn't it?

The Witness: It is lighter than the average of the reduced crude with which M is charged, but not lighter than the lighter constituents.

The Master: You spoke of that light material passing from the still M through the pipe line from the bottom of it which is not marked. What do you say as to the practicability of that light oil being drawn from the descending pipe, coming out of the tower into the draw-off pipe as light oil, and not as mixture of the entire charge in the tank?

The Witness: It would be a mixture of the entire charge in the tank, partly a light oil and partly the reduced crude. It is to be borne in mind that this is the cracking still, this still P. Still M is the distillation still, and we have that product from the cracking still going around and coming back to the cracking still, which was the question that was directly asked me this time. Certain portions are taken off. In fact, he calls attention in the exhibit that there are ten different fractions removed. I believe it is ten. Yes, ten different fractions removed besides some of the heavy products which may go as fuel oil or may go to the paraffin presses, depending on how the still is operated and the kind of crude that is used.

The Master: All the heavier portions of the crude are in M, are they?

[fol. 1393] The Witness: At one time, yes. Though, of course, they do not stay there, they come across and go into P.

The Master: Then what is the stock that P is operated upon.

The Witness: P would be operating upon a stock carrying gas oil, paraffin distillate, and the heavy portions of the crude oil, which we do not ordinarily distill—such as cylinder stock. Or, if we distill them——

The Master: All the bottoms?

The Witness: All the bottoms, yes.

The Master: And principally all the bottoms, is it not?

The Witness: Well, yes, principally, because the gas oil is ordinarily not as big a percentage as the bottoms.

In this connection it must be borne in mind that what he is after are salable products and all of the products, 1, 2, 3, up to 10 here are essentially salable products, or products which on very minor treatment will be made salable products—that is, treatment with acid, or paraffin pressing or such as that. On the other hand, crude oil is not a very valuable material which is used as a feeding material; and gas oil, which is wanted to continue in the process and recrack, is still less valuable as a product than crude oil, it is worth less per barrel.

The Master: The point I was trying to get clearly in my mind is that the stock in the tank P, if there is any gas oil in it at all, is not unmixed as oil, but that it is the heavy bottoms of the crude with the gas oil in it, whatever gas oil there is in it.

The Witness: That is correct.

The Master: And those are the coke making constituents, those bottoms?

The Witness: Yes, but gas oil, of course, make some coke.

The Master: I understand.

Q. Doctor, I wish you would refer to Burton patent No. 1105961, Exhibit No. 174, and trace the course of the pressure tar, and any distillates obtained from it in this patent?

A. If your Honor will turn to Burton patent No. 1105961, there are two figures in this patent. Fig. 1, represents the

the cracking still, condenser and receiver; Fig. 2 is an atmospheric pressure still for redistilling the product, which is then collected in vessel 20 at the bottom and used as feeding stock again for the vessel 3 in Fig. 1.

The Master: Logically, Fig. 2 should have been Fig. 1, and Fig 1 should have been Fig. 2, following in succession. [fol. 1394] The Witness: Yes, I would say so. It might be taken that way.

The Master: That is, numbered progressively with the operation; is that right?

Mr. Wiles: Well, he starts assuming—he starts on the pressure still.

The Master: Yes, all right.

The Witness: Starting in the pressure still, as the patent gives that first, the oil is put into the vessel 3, heated up—

The Master: Crude?

The Witness: And cracked.

The Master: Crude?

Mr. Wiles: Gas oil.

The Witness: Gas oil or a mixture. It is heated up, and after a period of time, roughly a day and a half or two days, the operation is completed and the mixture of gasoline and kerosene is obtained in vessel 8. The residue then from the tank 3 of Fig 1 is taken and placed in the tank 1 of Fig. 2, and at atmospheric pressure it is heated there and distilled down to dryness—that is, to coking. The product so obtained is collected in vessel 20, and then for a later run is used again in the vessel 3 of Fig 1, either by itself or mixed with fresh charging stock.

Q. Doctor, what is a critical temperature?

A. A critical temperature is the temperature which if a material is heated, which material does not readily decompose—if that material is heated and it will form a vapor independently of the pressure. In other words, we have a temperature above which most of our liquids cannot exist as liquids, but only as vapors.

To illustrate that, I have here a small tube with heavy walls, roughly two-thirds filled with liquid carbon dioxide. You can see when you get pretty close up, the liquid level of the colorless liquid. This is carbon dioxide gas which has been liquified under pressure.

Q. That is now below its critical temperature, is it?

A. It is now below its critical temperature. I will now warm this, and you will note you can no longer see the miniscus in it at all. It is now above critical temperature, and I may add it is under a quite high pressure, the pressure being more than a thousand pounds per square inch.

If now we cool this slightly we will have a condensation of the materials, of the carbon dioxide and we now have the liquid showing again. If you looked at it, you may have no- [fol. 1395] ticed at the start, on cooling it, we had gas here and liquid above. In other words, the two were of approximately the same density, and we had for a brief period of time liquid floating on top of the gas. It is what I have said regarding carbon dioxide having a critical temperature near room temperature. In this case it is about 30 degrees Centigrade, which would be 86 Fahrenheit. At any temperature above that it is impossible to have carbon dioxide as a liquid no matter what pressure it put upon it, whether 1,000, 2,000 or 10,000 pounds per square inch. But even a tenth of a degree below its critical temperature we can make it a liquid by putting on a pressure of, roughly, 1,000 pounds per square inch.

Similarly, we have critical temperatures for various liquids—water, ether, the pure hydrocarbons of the petroleum series.

Q. Have the critical temperatures for petroleum products been determined?

A. Yes, for some of the constituents, and indeed for mixtures as well. We know the critical temperatures for typical gasoline and for kerosenes, for gas oil, and they can be determined without any great amount of trouble. It is not any more trouble to determine the critical temperature of one of these products than it is to make, for example the bromine number or an iodine number for the oil.

Q. Is one gasoline like another in respect to its critical temperature?

A. They will vary to some extent, but they are in the same general range.

Q. Have you made determinations of critical temperatures of gasoline and other petroleum products?

A. I have, and I have had my students make them.

Q. Does a material have to be pure material in order to have a definite critical temperature, or can you determine the critical temperatures of mixtures such as gasoline, kerosene and gas oil?

A. It does not have to be a pure material in order to have a critical temperature. We can determine and it has been done, the critical temperatures of mixtures such as gasoline or kerosene, or gas oil, or mixture of those products.

Q. Does a critical temperature bear any relation to the boiling point of the product?

A. Yes, it bears a close relation to the boiling point and indeed, for petroleum hydrocarbon, such as in this group of gasoline, kerosene and gas oil, it can be determined by a careful determination of the boiling points for each portion of the oil.

[fol. 1396] The Master: In language that a layman can understand, is the critical temperature the temperature at which the fluid begins to vaporize or——

The Witness: No, it is the temperature——

The Master: At which decomposition begins or what?

The Witness: No, it is the temperature below which it is not possible to have it as a liquid; it will be a vapor there. And you cannot under any circumstances above that temperature have it as a liquid, no matter under what conditions. Except by cooling it. Then you can get it below that temperature and condense it to a liquid just as I did the carbon dioxide by cooling it below thirty degrees Centigrade or 86 degrees Fahrenheit.

The Master: What is the critical temperature of water?

The Witness: I do not recall. I can give you one or two others.

The Master: Never mind.

The Witness: Ether I can give as pure material, I think. I should say that water, as I recall it, is around 900 degrees Fahrenheit. It is quite high.

Q. Given the ordinary American Society for testing materials distillation curve for one of these oils, such as gasoline or kerosene, can the critical temperature be calculated?

A. Yes, and quite accurately.

Q. On page 2731 defendants' expert said:

"I do not know of any commercial mixture where we know enough about the hydrocarbons which are present

and the relative proportions of the different hydrocarbons, and the critical data of the different hydrocarbons, to make it possible to calculate within a hundred degrees or more, if that closely, what the critical temperature of a given mixture would be."

Is that statement correct?

The Witness: No, we can calculate it within a very few degrees. In fact I have before me calculations for gasoline and kerosene from different oil fields and then the observed figures of the actual determination, and in general they agree with each other within—well, here is one series of twelve in which all are within one per cent and most of them are within .4 per cent of the calculated temperature.

Q. As you crack a gas oil, for example, to give lower boiling products and at the same time higher boiling asphaltic products, how does a critical temperature vary; is it increased or decreased?

The Master: That is as the operation is going on?

[fol. 1397] The Witness: Yes.

A. It would be decreased. That is, the more you crack your products, even though you are cracking some higher boiling products you get more lower boiling products, and the critical temperature of the mixture is decreased, from what it was in the starting material used.

The Master: You mean the critical temperature of the residuum?

The Witness: Well, I was taking the material carrying the cracked product with it. In other words, if you would take, say, in a Cross process, at the beginning of the tube take out a sample of the oil and determine its critical temperature, take it again when cracking has begun and it will have a lower critical temperature, and at the end of the tube where a cracking is very decided it will have a still lower temperature; and as it comes out from the reaction chamber it will have a still lower temperature—the mixture will.

The Master: You are referring to critical temperature and not actual temperature?

The Witness: Critical temperature. It would be, in fact, increasing in actual temperature up to the end of the coil, and then decreasing afterwards.

Q. If you heat a gas oil in a coil or tube to 850 degrees, would it be in a liquid condition or in a gaseous condition under any pressure you might put on it; which would it be in?

A. There would be cracking beginning when it had reached around 700 degrees, and by the time it had reached 850 degrees there would be considerable cracking, and it would be considerably above its critical temperature and be there exclusively as vapor, as gas and not as liquid.

Q. Regardless of the pressure that you might put on it?

A. Regardless of the pressure that you might put on it. I say exclusively; but that is not quite correct; there is a little carbon dust due to the cracking that would not be dissolved in the vapors. But even the asphaltic materials would remain dissolved in the vapor just as they would in the liquid. And so even all our heavy materials would be dissolved there.

Q. Dr. McKee, when you draw a sample of a gasoline from a condenser operating under pressure, it would foam some, would it not?

A. Yes, it would give off gas, the dissolved gas and foam.

Q. Suppose you were to pour a very light gasoline obtained from an atmospheric pressure condenser—put it into a large graduate or beaker, would it also form bubbles similar to a foam?

A. It would, and the appearance would be very similar, each of them a good deal like soda water in appearance.

Mr. Rauber: I have a disclaimer here, which I wish to offer in evidence. This is going in by stipulation with Mr. Clark. It is a disclaimer to claim one of patent number 1410797. I offer this as an exhibit. That patent is Exhibit No. 191.

The Master: Claim one is disclaimed?

Mr. Rauber: Yes.

Mr. Wiles: That is the only claim of that patent that is discussed here, so you can just mark that out.

Mr. Rauber: Yes, it can go out.

Mr. Wiles: That is the only claim of that patent discussed here, so you can mark that off your list.

(Whereupon said exhibit so offered in evidence, was marked Exhibit 605, read in evidence and the same is returned herewith.)

[fol. 1398] Cross-examination.

By Mr. Wiles:

Q. Was I correct in understanding your direct testimony this morning to the general effect that you cited Robinson's book on the proposition that the better practice is to put the reflux in below the upper pan of a bubole column?

A. Yes.

Q. And you think that is a fair interpretation of that book that you made to the court—an honest, fair interpretation?

A. Yes.

Q. You do?

A. Yes.

Q. Have you the book here?

A. No, I have not.

Q. I call your attention to the language at the end of page 52, and I will then ask you again to state whether you think you made a fair, honest interpretation:

"The best column should have its reflux entirely in the condenser above the column and the entire reflux returning through the entire length of the column."

Verify my quotation (handing witness book).

A. I do not know whether this was in the edition which I had or not, but it was the first time I have seen it. What edition is that, may I ask?

Q. First edition, second impression.

A. This is first edition, third impression. This is the first time I have seen this and I am not at all certain that this portion on page 52 is meant to say that the illustrations which are put in on pages 116, 117, and in fact largely through the book—pages 139, 147—are illustrations then of poorer practice than what he recommends.

Q. Now, will you just answer the question, is the matter I quoted to you in the book yes or no?

A. It is, on page 52.

Q. Answer this question: Is the quotation that I read to you immediately preceded by a considerable mathematical

demonstration of its correctness? Just answer that yes or no.

A. No, only a very short paragraph in which he is discussing not the problem which you are mentioning, but the question of column insulation.

Q. Does that, or does that not bear on the question as to whether the reflux should traverse the entire length of the column? Please answer that question yes or no, if you can.

A. Only very indirectly. It bears, primarily, on the amount of reflux. For example, he states here that the fractioning in the column will be greater, the greater the amount of reflux, and that is what this mathematical phase is concerned with.

Q. Now, have you the book—the first impression, or the second impression that you quoted from?

A. It is in Mr. Rauber's office. It happens to belong to Mr. Rauber, and not to myself.

Q. Will you bring it in?

A. Tomorrow.

Q. Yesterday, on page 9350 of the Typewritten Transcript, you were asked this question:

"Defendant's expert also stated that Adams was the first man who, in a bulk pressure distillation process for making gasoline, disclosed the application of heat to the intermediate portion of a vertical cracking still, below the liquid level."

On page 9351, you seemed to have answered the question:

"Well, it is in Schuchow and Gavrilow and Kornfeld."

Did you mean that answer to apply to that question?

A. Would you give me the page?

[fol. 1399] Q. Pages 9350 and 9351. There was some matter came in between, and I want to make sure that that was meant to be an answer to that question.

A. Yes.

Q. And you were questioned, after that, about Schuchow, but not about Kornfeld. You think your answer with reference to Kornfeld is correct, do you?

Mr. Wilson: "Exhibit No. 123."

Mr. Wiles: Page 1570.

A. I so thought at the time, and particularly in connection with the last paragraph, on page 1574, where the collector is placed outside of the furnace. He says:

"I can also arrange my boiler in such a way that the tubes, *a*, are alone enclosed in the heating chamber, while the collector will be placed outside of this chamber."

The foot of the tube is also outside of this chamber.

This would be correct, unless the question arose as to where the liquid level is.

I thought that there was a statement in the text regarding the liquid level, but I don't find it, now.

I do note that in the figure given there, there is apparently a liquid level below the top of the chamber.

Q. In the fire space?

A. Yes.

Q. So that it fires both above and below the liquid level?

A. Yes.

Q. As illustrated?

A. Yes—where the chamber is within the fire space.

Q. And that would be equally true, if you moved the collector outside the fire space?

A. It might, or it might not.

Q. In this patent you say that there is not one word in the text to show where the liquid level is?

A. I didn't say that.

Q. You said you couldn't find it.

A. I was under the opinion that it was there, but I have not been able to find it just now, I said.

Q. All right. Please do.

A. I do not find it, as I thought it was there.

Q. Then the only indication as to the liquid level is that in the drawing—isn't that true?

A. Yes, excepting the statement of the feeding of the pipes at their lower point, which would imply, of course, that there was liquid in the pipes.

Q. But it doesn't tell you anything about the liquid level?

A. No, not in its operation.

Q. Then, based on the information given in the patent, your statement, yesterday, to the Court, was not true, that Kornfeld had the things that were stated in the question that you were asked on page 1950? Yes or no—was it true, or not?

A. My answer was, in major part, true. Only one word is untrue, out of a total of something like fifty words.

Q. In other words, you told the Court that Kornfeld made that description, and it does not—that is the fact, isn't it?

A. And I believe it——

Q. Is that true, or——

A. That statement is true, but I told the Court, also, that Schuchow and Gavrilow, and Warden made the statement, except that Warden was amospheric, which is true, also.

[fol. 1400] Q. Now, you cited, this morning, Redwood's book as showing some distinction with reference to cracking processes on shale oil and petroleum. You did not intend to indicate that the chapter on cracking was in any way so limited, did you?

A. No. I should say that the chapter on cracking would apply to either shale oil or to well petroleum.

Q. And it does so apply, and most of the examples given are cracking of shale oil?

A. No, they are not.

Q. Redwood first gives Thorpe and Young's process, on page 1384. That is on a shale oil product, isn't it? He specifically says it was on paraffin produced from shale.

A. No The first one is 1384, Professor Silliman—there is a discussion there on the question of American petroleum, on fractional distillation. That is on petroleum.

Q. That is the first one. Now, the second one is on shale?

A. The second one is Thorp and Young. The third one is Riebeck, which worked on Baku petroleum, as is stated there.

Q. Also on lignite, which is a coal distillate, isn't it?

A. Yes, sir, but not a shale. Leet, on page 1385, describes the cracking process at Newark, New Jersey, which is a well petroleum process.

Vieth's, on page 1385, is a discussion of cracking of oil petroleum—not shale petroleum.

Page 1386, we have the Young process, and that was for shale oil.

On page 1386 we have the Benton process, which was for well petroleum.

Q. And the last one is Dewar and Redwood?

A. Yes, Dewar and Redwood—that is for both. So that we will put it as double. On page 1389, a Russian one, for cracking process on petroleum. So that we have, not what you stated, but the truth is that there are seven for petroleum and three for shale.

Q. And one for coal?

A. One for coal.

Q. Now, you said yesterday that the Riebeck process was run on a commercial scale, for the manufacture of gasoline—I believe—is that correct, and does that correctly state your position, if you didn't say it just that way?

A. Not entirely. It was run for the manufacture of illuminating oil and gasoline.

Q. And on what you called a commercial scale?

A. Yes, sir.

Q. Now, that commercial scale was in stills of 170 gallons capacity, was it not?

A. Can you give me the citation for that?

Q. It was one of the latest exhibits. Page 4246—500 kilograms.

A. Which you say is 170 gallons?

Q. That is right, isn't it? Someone put that on the margin of my book. That is approximately correct, isn't it?

A. I think so.

Q. Now, that still was turned over two and a half times in twenty-four hours—that is correct, isn't it?

A. Yes, sir.

Q. So that it would run 425 gallons of oil?

A. That is right.

Q. Now, the final preferred process that they wound up with, is described on page 4228, isn't it?—as their commercial process, where they say, "After operating the apparatus for some time, and when treating a heavy dark paraffin oil of 0.900 specific gravity, I now work with 60 per cent. pressure distillate 0.830. There remains 30 per cent. residue, the remainder of 10 per cent. is lost as water, gas and coke," and then they continued to describe that final [fol. 1401] method—that is right, isn't it?

A. I don't think that that is the final one. I think there is a later citation, which gives more, but this is the operation at the time this article was written.

Q. And that was the operation that, after some experiment, they had come to prefer, was it not, and that is the one that you meant was commercial.

A. This was a commercial plant, at that time.

Q. And that is the operation of that plant which you said was commercial.

A. Not the only operation. Other times they ran——

Q. That was one of the ones, then, that you said was commercial?

A. Let me finish my answer, please. That was not the only operation which was said to be commercial. He speaks, just above there, of taking off other proportions of distillate.

Q. But was there one of the operations which you said was commercial?

A. Yes.

Q. Now, turning to page 1229, I find that that operation produced approximately six per cent. of benzine per day, per still, or .75 gravity. Now, just verify my figures. That amounts, does it not, to 25½ gallons of gasoline of a Baume gravity of 57.1. I want merely a verification of those figures.

A. The figures as given are not quite correct.

Q. How much do they differ from being correct?

A. A very decided amount, in the gravity of the benzine. The 57 is very different from .75.

The Master: How much in quantity?

The Witness: Not in quantity, but he is giving it here as if it was a very light gasoline, .57, whereas it is .75.

Q. .75 specific gravity, which is, in fact, 57° Baume, is it not?

A. That wasn't in the question.

Q. Maybe I misstated it, and if I did, I will ask you now, if that was not 25 gallons per still per day, of 57.1 Baume gravity gasoline?

A. It is.

Q. Now, you told us yesterday—without citation of any authority—that the Young pressure cracking process was operated commercially. Please state what you know of your own knowledge, on that subject.

A. I was not alive when Young's invention was made but my knowledge comes from having visited the plant and being told by the then manager, of its use, and of since having had it confirmed by the assistant manager, Mr. Conacher, whose article I quoted here, from one of the scientific periodicals, this morning——

Mr. Wiles: Your Honor, the answer is objected to as not responsive, and as volunteered and I move to strike.

The Master: I will not strike it out, but it is hearsay.

Mr. Wiles: Of course it is objected to on that ground, if it stands, because I ought to have a right to cross-examine, about something he knows.

The Master: Of course the witness gains nothing by endeavoring to insist on going on when he has answered the question.

Q. Now, do you know of any printed publication citing any commercial operations of the Young process—the Young pressure cracking process?

A. I know of printed publications describing the result [fol. 1402] from the Young pressure cracking process, but not of the technical side of the process.

Q. The publications you refer to are in evidence?

A. No, they are not in evidence.

Q. What are the publications?

A. The publications, or publication, was a publication of an English company, which was using the process, in an annual report, or something of that kind, which was shown to me at the time, in 1920, when I visited the Scotch shale oil plant.

Q. Can you secure a copy of that for us?

A. I cannot. So far as I know there may be no more than the one copy, or a few copies which were available in the files of the Scotch company. They were quite old.

Q. Now, Young also was the patentee of a very celebrated process which was widely used, was he not?

A. This process is a quite celebrated——

Q. My question is, Young was the patentee of the original shale oil patent process, which went to the House of Lords, and was extremely well known—was he not?

A. I would have to ask whether you refer to the process in which there was quite a voluminous lawsuit?

Q. That is the one—this is the same Young, isn't it?

A. I think so—I am not certain.

Q. And that process controlled the whole shale oil industry, for a long time—during the life of the patents?

A. No, because it had to go up as a lawsuit, in order to be fought over, as to what oil shale was. There were people who were using the process which Young's patent claimed to control, and others said it did not.

Q. Well, the patent was sustained?

A. I don't recall in that respect.

Q. Now, did I correctly understand you to say that Dewar and Redwood did not disclose the running of reduced crude in their process?

A. I don't know whether you understood it, or not, but——

Q. Well, was that your——

A. I don't think that I so stated. I don't recall making any such statement.

Q. Well, if you did say it, you didn't mean to say it?

A. The patent itself, gives——

The Master: Can you answer the question without going back to the patent?

Mr. Royce: He is asked here, your Honor, if he said something which he doesn't recall saying, and in answering the question I think he needs to refer to the patent.

The Master: He has been studying these patents for two years, now, and he is asked a question of that kind, about the Redwood patent, and he ought to be able to state it without going back and reading over the patent again.

A. (continued). I would say that the disclosure on page 1389, where it speaks of the materials which may be used for this, as intermediate, and as heavy oils—that the alternate heavy oils might be reduced crudes.

Q. And, furthermore, in the first two or three paragraphs of the patent, he expressly mentions reduced crude, does he not?

A. I was not reading from the patent. I was reading from exhibit hundred and seven.

Q. On page 9247 you said, referring to Dewar and Redwood, "So they do disclose the use of a crude oil, but not a reduced crude oil." You say now, do you not, that that statement is incorrect.

A. That portion of the statement I now believe to be incorrect, in view of re-reading the portions which I quoted to you a few moments ago. The other portion of the answer is correct—the preferred material being intermediate.

Q. Now, when you look at the first part of the patent, the specific material that they mention is reduced crude, is it not?

A. In "Exhibit No. 103," that is a reduced product from either natural petroleum, or shale, or coal, or other bituminous substance. "Exhibit No. 107,"—which was the other—they speak there of intermediate oils, which was the later interpretation by Redwood, as to preferred material.

Q. In the first part of the patent—I want to get my [fol. 1403] question answered, if I can, and it will save lots of time if you will answer the question—in the first part of the patent it is a fact that the material mentioned is reduced crude, is it not? Please answer the question yes or no.

A. Crude what?

Q. Reduced crude oil.

A. If, by crude oil, is understood shale oil, coal oil and other bituminous oils, as well as well petroleum, that is correct.

Q. And that is a product heavier than crude oil?

A. Yes.

Q. Crude oil, in turn, is a product heavier than the light distillates from crude oil—kerosene, for example.

A. Yes, and gasoline.

Q. And they also disclose, you said, the running of crude oil, as well as reduced crude oil. You said that on page 9247. You don't want to retract that, do you?

A. That is the same sentence there, and I can't help but wonder whether it wasn't a slip there, in which the two phrases were exchanged.

Q. I don't care how it got there, but I want the facts, Doctor. Of course the fact that I got it in my notes indicates that you said it.

A. I say it might have been a slip of the tongue. I was not questioning the stenographer's transcript.

Q. Now, you state that you made some tests on the Cross process. Tell me where that was and when.

A. It was last spring—part of it in the Cross laboratory,

at Kansas City, and a part of it at Columbia, in New York City, in my laboratory.

Q. Now, you said you tested some material as it came out of the Cross coil. Where was that?

A. I didn't quite say that I tested it as it came out. I tested the products which came out.

Q. Now, did you take it out?

A. No, I didn't say that I did. I had it taken out and——

Q. Now, that is enough. You didn't take it out. Now, will you just answer the question, and not something else. Did you see it taken out?

A. I didn't see it taken out. I ordered it taken out, and——

Q. Just a minute. That is enough.

A. I beg your pardon—I did see it taken out in one case, and took it out myself.

Q. Just when was that, and where?

[fol. 1404] A. At Kansas City, about around June 15th, 1926.

Q. Just what plant was that in?

A. That was in the demonstration plant of the Cross laboratories—the Kansas City Testing Laboratories, which are run and owned by the Cross brothers, of which Walter Cross, the patentee of one of the patents in suit, here, is one of the two brothers.

Q. That was not a commercial plant?

A. No, that was not a commercial plant.

Q. It was a laboratory plant?

A. No, I should hardly say it was a laboratory plant.

The Master: You just said it was part of the laboratory plant.

The Witness: No, I said it was the Kansas City Testing Laboratories—that is the title under which it goes.

Q. What was the capacity of that plant?

A. Oh, it was of insignificant capacity. It was a demonstration plant.

Q. How big was it? You said it was insignificant—what was it?

A. As I recall it, Mr. Hoguet gave the weight of it as something like fifteen hundred or two thousand pounds.

Q. Total. How long was the coil?

A. Only a few feet.

Q. And how big was the drum, or the reaction chamber?

A. Some four feet in length, or maybe five feet in length.

Q. How big in diameter?

A. Three or four inches—about three and a half.

The Master: Internal diameter?

The Witness: Internal diameter—3½ to 4 inches internal diameter.

Q. And just where did you take this oil out, that you took out? Confine yourself to your own sample.

A. The samples that I took out from that, were not from the—they were samples of products made under varying conditions, and——

Q. I didn't ask you that. Where did you take out the sample that you have said you took out?

A. Kansas City, Missouri.

Q. State what part of the apparatus did you take out the sample that you personally took out.

A. At the outlet from the condenser.

Q. I was asking you where you took out a sample, at the coil.

[fol. 1405] A. But you changed your question and made it where I took out a sample from the Cross process, and I answered that.

Q. All right, and I will have to go back all over it again, if that is the theory upon which you answered. Did you, yourself, take out any sample, at any time, of the oil, from the Cross heating coil?

A. Of a commercial plant?

Q. Any plant.

A. It is hard to say, in a case like that of the Kansas City plant. The upper part which they called the chamber, was heated—which it was not, in commercial plants—in order to avoid radiation—I mean, avoid cooling—so that if you define the coil as the pipes, there, they were heated, and then it would be—then I took out the sample there at Kansas City, about the middle of June, 1926.

Q. Did you take any sample, at any time, from the coil which precedes the reaction chamber in the Cross process?

A. No, not personally. I——

Q. No, that is enough. You have answered the question.

Now, in the operation of the Cross process, then, which you saw in Kansas City, you don't know, of your own knowledge, what were the conditions as to liquid and vapor in the heating coil.

A. I do.

Q. Did you see those conditions?

A. The temperature measurements were taken as to the temperature in the heating coil. I took some of the same material, which was used as charging stock, and found that the temperature which they were using was quite a good deal above the critical temperature of the stock, which they were using, even though there had been no cracking, and as cracking went on it would have been still more above the critical temperature.

In other words, it must have been in the vapor phase, and there was certain other observations made at that time, at the plant.

Q. Now, you cannot know, can you, of the condition of the stock, or how it had been changed in the coil?

A. Certainly I could tell.

Q. How could you tell?

A. Because as it passed along it was getting more and more cracked—cracking doesn't occur only at a particular stage—and, independently of that, the stock, itself, had a [fol. 1406] critical temperature below the temperature used in the coil, as recorded by the pyrometer.

Q. Now, just tell me how you derived this critical temperature—in detail.

A. In that particular case I took a sample of the stock which was fed in as it was being fed in, and brought it back, sealed up in a tube similar to that in which the carbon dioxide was sealed, which we have here, but somewhat smaller—and then heated it and determined its critical temperature by measuring the temperature to which it had to be heated, in order to determine noting the point at which the product passed from the liquid to the gaseous phase.

Q. Now, what pressure did you have in that tube?

A. I had pressures, so that we had to—I had the tube filled approximately at, a half or a little more, full, and that would mean that we had pressures of very considerable extent. You can tell, moreover, whether you have the crit-

ical pressure, or not, by the phenomena that takes place in the tube, on cooling—from the vapor phase.

Q. You didn't measure the pressure?

A. No, excepting indirectly. I did fill one or two of—in one or two of the several experiments that I carried out, I did fill one or two tubes too full and blew them up, when the liquid would expand and completely fill the tube, before it changed into the gas phase.

Q. Are you able to say that a heavier pressure would not have liquified the product?

A. Yes.

Q. How?

A. If that had been true, then on cooling, as the tube cooled from above its critical temperature—it would have precipitated the liquid and it would have run down the sides of the tube to the bottom, and not cooled, and precipitated into a liquid condition, away from the vapor condition.

Q. When you have mixtures of liquids like these it cannot all go down to liquid at once, can it?

A. It does.

Q. The critical temperature of the several ingredients is not the same, is it?

A. No, but you have a critical temperature of the mixture—that is a definite temperature. For example, if you mix gas oil and gasoline, your critical temperature is away below the critical temperature of the gas oil.

[fol. 1407] Q. Now, there is a critical pressure, is there not, as well as a critical temperature?

A. Yes, sir. It is the pressure, which a liquid gives at the instant it changes from a liquid to a—which a liquid gives at its critical temperature.

Q. Unless you have a pressure in your tube above the critical pressure, you cannot be sure that you have reached the critical temperature—isn't that correct?

A. That would be correct, but I have also other ways in which I know that we were above the critical pressure.

Q. Well, furthermore, it is a fact, with reference to all these critical temperature phenomena, that if you have impurities present you may have a very different result from that with a pure substance—isn't that true?

A. It is true that mixtures give results, different from pure substances. For example, I mentioned, a moment ago, that a mixture of gasoline and gas oil will give a figure away below the critical temperature of gas oil and considerably above the critical temperature of gasoline.

Q. In other words, you can hold gasoline liquid away above its critical temperature, if there is any gas oil present.

A. Yes, above it.

Q. And the same is true of your carbon dioxide—if you added a little water in there, it would have been liquefied in the water?

A. No. It is not soluble. In the case of carbon dioxide that amount of carbon dioxide would not be soluble in the water.

Q. A very large part of it would have gone into solution, wouldn't it?

A. No, a very small part. It is only soluble at ordinary pressures and temperatures, about 3 or 4 per cent.

Q. By weight?

A. By weight—that is, 100 grammes will only dissolve 3 or 4 grammes of carbon dioxide.

Q. Sure—and that is a very large volume.

A. It is only an equal volume.

Q. And at the thousand pounds which you told the Master existed in that tube, a drop of water would have dissolved about all the carbon dioxide there, wouldn't it?

A. No. This is very well illustrated, Mr. Wiles, because in the carbon dioxide plants where they are liquefying carbon dioxide, they pass the carbon dioxide through the compressors and get out liquid water, which is drained off, and does not contain but just a little bit of carbon diox- [fol. 1408] ide, by weight. You can see that in such a plant as those of the Liquid Carbonic Co., or the General Carbonic Co., over at Long Island City.

Q. What is the critical temperature of asphalt?

A. Nobody has ever determined it, as a pure material. We do find—and this has never been published, but it is true that we do find that asphalt will appear in vapor form in these vapors when they are above critical temperature.

Q. Now, have you stated your full method for obtaining these critical temperatures, on which you base your testi-

mony that in the Cross process the oil is all vapor, at the end of the heating coil?

A. No. We have worked out a method by which, from the A. S. T. M. distillation record—a curve—and we can calculate that the critical temperature would be, and, as a check for that we have determined, and found that the two agree, within a very few degrees—not hundreds of degrees, as——

Q. I want you to just tell us what the method is that you have worked out. If you have a copy of your work I will take that.

A. I haven't a printed copy of the work. The calculated value can be obtained from the equation that the critical temperature equals 1.05 times the boiling point, plus 160. The boiling point and critical temperatures are given in Centigrade. This boiling point is the average boiling point, calculated by averaging the temperature of the first drop, and each of the ten successive cuts in the standard A. S. T. M. distillation.

Q. Where did you get that formula?

A. We worked it out at the Columbia laboratory—Ph. D. students.

Q. And that applies to any kind of a mixture?

A. So far as we have tried—and we have tried many kinds—it does apply to any kind of a mixture of petroleum products, of the ordinary types. We did try it, also, on some other things than petroleum products—like brome benzene, and it applies there.

Q. And on that basis what is the critical temperature of gas oil?

A. Gas oils will vary among themselves, according to their distillation.

A Vinton gas oil, from the Vinton crude, gave what figured out to be 439 degrees C.

[fol. 1409] A Bradford, Pennsylvania, gas oil, 487 degrees Cc. That is the highest that we have found of any of the gas oils.

The other oils that I have here, are not strictly gas oils, though some of them are portions of gas oils, such as 300 oil and others—kerosenes, of course, and gasoline having lower critical temperatures.

Q. On the ordinary kerosene mixture, it would be what?

A. Around a figure of 405 or 406, Centigrade. I may add

that the observed ones, when we carried out the experiments —it came within—one was, where it was calculated as 405, it gave 407. That was a Burbank kerosene. A Bradford kerosene, where it was calculated at 406, came out 405. A Pioneer field kerosene, where it was calculated at 405, came out 407. A Vinton one, calculated at 384—and in that case we had the largest variation—it missed it quite a little bit—it came out 400.

The A.S.T.M. curve, though, is an irregular curve, and not as smooth as most others.

Q. So that practically all of these cracking processes are running right on the edge of the critical temperature, according to your theory?

A. No. The starting material is on the edge, but as soon as you have cracking taking place, you reduce the figure by a very considerable amount.

For example, a Bradford, Pennsylvania oil, that had a critical temperature of 458, gave, when we put gasoline in the product, we got the equivalent of a cracked product, and it gave 381, calculated, and actually came out as 380 degrees, by actual experiment, when actual measurements were made. And that, of course, is very much lower than the temperature there in the cracking apparatus.

Q. So that practically all of these cracking processes are right, as I said, on the edge, or over the critical temperatures—either the tubes, or the other ones—according to your theory.

A. I don't understand you.

Q. For instance, a vertical Burton still—take this Bradford oil that you have just given—that is 381, when it has been cracked a little.

A. No, it wasn't cracked a little. It had 50 per cent. of gasoline in it. That was an artificial mixture of 50 per cent. Burbank—no, I said that was Bradford—I want to change that—it was Burbank—it had a 50 per cent. of oil that had [fol. 1410] a critical temperature of 458, and 50 per cent. was all that had a critical temperature of 305.

Q. Is that the figure on which you say the critical temperature is reduced when you partially crack?

A. No. This would be an artificial product, but I have also tried products which are partially cracked, and the greater the cracking the lower the critical temperature.

Q. Now, you gave this illustration to give quantitative

figures on the amount of reduction, as I understand it, did you not?

A. Yes, sir, on the question of a mixed product.

Q. I didn't ask that. How much does the critical temperature come down when you crack?

A. Oh, in the order of about 50 degrees C., in most cases, which would be 90 degrees F.

Q. Then these gas oils are down around 400, after they have begun to crack a little—that is, their critical temperature is down around 400?

A. No, I wouldn't say when they began to crack. They would gradually go down to that, and when we got at—400, or possibly some of them below, and some of them above——

Q. The whole crude is around 400, of gas oil, after it is cracked some.

A. I don't know just the extent you mean, by "some".

Q. Well, suppose it has been cracking in a Burton still, we will say, for 4 or 5 hours.

A. I wouldn't think so, in that case, because you would have distilled off your gasoline and kerosene, on that, and you would have a product which would be the residual product, and probably be even higher than the original gas oil, and if you would give me an A.S.T.M. distillation of that, I could probably figure and tell you exactly, but in the Cross process, or tube and tank, or Holmes-Manley, you have other conditions.

Q. Now, at the start a Burton still, running in the ordinary way, would have its temperature extremely close to the critical temperature, would it not?

A. No. As I understand, you run your Burton stills at around 720, and the critical temperature of a gas oil, if you figure it in Fahrenheit, would be around 850.

Q. So that it is about 120 or 130 degrees below the critical temperature.

A. Yes, sir.

Q. And, of course the latent heat, under those conditions, is very, very small.

A. Which do you mean—under the critical condition? At the critical condition it is zero.

Q. Of course it is zero at the critical temperature, but under the conditions I have stated your latent heat is extremely low, is it not?

A. I don't recall how the curve is for latent heat, but it is not a straight line curve.

Q. But it has got to drop right off, as you go towards the physical temperature.

A. Yes, sir, and it drops, I think, pretty rapidly at that last portion.

Q. So that it has got to be an extremely low latent heat, under the normal conditions prevailing in the cracking process.

A. No, I don't think that is correct.

[fol. 1411] Q. Dr. McKee, you testified yesterday afternoon on cross-examination about certain samples that you drew and certain operations you watched with a Cross apparatus in Kansas City. Do you recall the occasion of that testimony?

A. Yes.

Q. Do you remember what it was about?

A. Yes.

Q. Look at the drawing I now hand you and state whether or not that is the drawing of the apparatus that you referred to as a Cross apparatus in the testimony yesterday afternoon (handing witness paper)?

A. It is, and I should add that it is——

Q. You do not need to add anything. That is all I have asked you.

A. I would like to ask something.

Mr. Wiles: I offer that drawing in evidence as Exhibit 610.

(The drawing so offered, was admitted, and read in evidence, a photostatic copy of the same was marked Exhibit 610, and the same is returned herewith.)

Q. On page 9254 of the typewritten record you were asked about the relative efficiency as fractionating devices of the Hupmphrey's run-back and the stillhead on the Dewar & Redwood still. In your answer beginning on page 9255 of the typewritten transcript of the record you quoted a paragraph from Young's "Distillation Principles and Processes," beginning on page 127. Then you skipped and quoted another paragraph, which is at page 9257 of the typewritten transcript of record.

Do you consider that you by those quotations fairly and honestly reflected Young's views with reference to a reflux device of the character shown in Humphrey's patent?

A. Yes, so far as compared with Dewar & Redwood. Young does, however, state——

Q. Just a minute. That is all I want. You think that those quotations fairly and honestly reflect Young's views——

The Master: Do not go beyond the question, doctor. When you have answered the question, stop.

Q. (continued). —and do you think you have told the Court the whole truth with reference to Young's views on the matter you were then testifying about, yes or no?

A. Your Honor, that question cannot be answered with yes or no only.

[fol. 1412] Q. I first hand you a photostatic copy of the title page and page 127, being the first page of that book from which you quoted, and the first paragraph on page 127 is the matter which you then quoted, is it not (handing witness paper)?

A. Is part of the matter which I then quoted.

Q. That is the matter that you quoted, from page 127, is it not, and all of it?

A. It is what I quoted from page 127.

Q. Yes.

A. But I quoted from elsewhere.

Q. I hand you a photostatic copy of page 131, from which you also purported to quote (handing witness). The matter which you quoted on page 131, is that part immediately following the large letter general subject heading "Modifications of the Still Head," is it not?

A. Yes.

Q. And that is all that you quoted?

A. Yes.

Q. But you did not, however, quote the large paragraph heading "Modifications of the Still-Head?"

A. I don't recollect I did or not, but the record would show.

Q. Both of those quotations have to do, have they not, with plain vertical stillheads?

A. Yes, in which it is said——

Q. Vertical cylinders?

A. Yes, as I read them. They were cylinders vertical, such as the Humphrey's, except that the Humphrey's is inclined.

Q. You did not quite the succeeding paragraph which commences one line beyond where you ended, that is entitled

"Sloping still head," and distinctly referred to the kind of general type of fractionation on the Humphrey's device, did you?

A. I did not quote it. It was however,—may I explain further?

Q. That is all. I am not interested any further. You did not quote it. And you did not quote it even though it is perfectly evident from the heading of the paragraph that you did quote that that is an introductory paragraph to something that follows.

A. But there were a number of pages that followed. I did not attempt to quote all of it, discussing the question of how inefficient the plain vertical and the sloping plain were as compared with the bulb forms.

Mr. Wiles: I offer the title page and pages 127 and 131 of this book as defendant's Exhibit No. 611.

The Master: They will be marked as one exhibit.

(The said title pages, so offered, were admitted and read in evidence, and a photostatic copy of same was marked Exhibit No. 611, and the same is returned herewith.)

Q. You have repeatedly said that the Dewar & Redwood still head is a bulb. Does the Dewar & Redwood patent say it is a bulb?

A. No, but it diagrams it as a bulb.

Q. That is all. Now, just a minute, if you will just answer my question. The diagram is a longitudinal section, is it not?

A. I do not think he so describes it.

Q. Well, what is it?

A. Figure 1 is a longitudinal section; Figure 2 is a transverse section; Figure 3 is a sectional plan.

Q. The only one of those views that shows the still head is a longitudinal section, is it not?

A. Yes.

Q. And that figure represents, does it not, just as well a transverse pipe as it does a bulb?

A. I do not think it does. If it did, it would have square ends.

Q. You could not put round ends on a pipe?

A. You do not.

Q. Could you?

A. I could not.

Q. The bulbs which are shown in Young's book and which you referred to, gradually flare out, do they not? That is, the vertical neck goes by exaggerated curve into the bulb?

A. Some of them do, some of them don't. He names quite a number of bulbs. The rod and disc, for example, is one which does not.

Q. I did not ask about the rod and disc. I asked about the bulbs.

[fol. 1413] A. The Wurtz form does.

Q. And the Dewar & Redwood does not; the Dewar & Redwood head has no such gradual flare, has it?

A. No, it is made of steel.

Q. And it is very common practice on steel pressure containers of cylindrical form to put round hemispherical ends, is it not?

A. Extremely rare.

Q. You have seen it done, have you not?

A. I was trying to think of a single case, and I cannot at present recall a single case where it has hemispherical ends.

Q. If you will just look at the still in the Dewar & Redwood patent, which is of the same diameter as the pipe that I think makes the stillhead, maybe you can find that kind of an end. Look and see if you do not find such a hemispherical end of what is plainly a type.

A. I said I do not recall having seen one. I am thinking of actual practice at the present day. I have seen it diagrammed, I have seen it on lead things, and I see it here in the diagram.

Q. And if the still head of Dewar & Redwood was a transverse pipe like the still, and had two ends like the right-hand end of the still of the same figure, it would be illustrated precisely the same way as it is in fact illustrated; that is correct, is it not?

A. With all those "ifs," yes.

Q. You testified yesterday—I cannot cite you the page, because I have not had time to correct my notes to the pages of yesterday's record—but it was necessary in the recovery of sulphuric acid to concentrate above seventy-five per cent, in fact necessary to concentrate above ninety per cent, around ninety per cent, and then dilute back, because otherwise the organic matter, hydrocarbon matter, in the sulphuric acid, would interfere in some way.

Just what is it that will happen if you do not concentrate up and remove these hydrocarbon bodies, but simply concentrate to the strength you want and leave them in?

A. I do not know just why the petroleum industry has found that it is undesirable to leave them in. I presume because we would have less action on the undesirable bodies in the petroleum f-action which is being treated.

Q. You said on page 9449 of the typewritten transcript of record, "They"—referring to these organic compounds —"must be decomposed before the acid is fit for use in [fol. 1414] treating hydrocarbon products, and it is necessary to concentrate to approximately 64 degrees Baume, which would be 85.7 per cent sulphuric acid, before we can get these compounds to act with sulphuric acid and be decomposed by the sulphuric acid.

Then on the next page you continue "It will not do to concentrate to 75 per cent acid and then attempt to use this acid on petroleum products such as gasoline or kerosene. It will not react satisfactorily and will not serve."

Mr. Royce: I want to point out the fact that you skipped "petroleum products" in treating hydrocarbons products.

Q. (continuing:) I want to direct your attention particularly, having read you this contract, to this sentence: "It will not react satisfactorily and will not serve." What basis of fact had you for making that statement to the Court?

A. My general knowledge of the petroleum industry as obtained in discussions with the parties who had been using acids for the treatment of petroleum products. I have no book to which I can refer you if that is what you mean.

Q. I asked you for a basis of facts. Now let me ask you another question. Do you know anything whatever about the truth of that statement, of your own personal knowledge?

A. Yes. And this proves it very definitely—that if you will take an acid, say, of eighty per cent and treat a cracked product with it, and use only a small amount of it you will bring about some resinifying, which does not use up sulphuric acid; you will bring about some chemical combination with the olefines and diolefines which does use up a part of it. If the statement you quoted was not true you

could continue shaking with that same material, and, even though its concentration had fallen down to 73 or 75 per cent, use it to complete the treatment. As a matter of fact you cannot do this, you have to use, if you are going to take out the olefines more completely, a fresh portion of acid. That is a definite proof of the facts which you requested.

Q. Then you say, as a matter of fact, within your own knowledge you cannot use recovered acid which is brought up to only 75 per cent strength?

A. Not if you use the modern method of concentrating acid, such as this more economical method is.

Q. And that is because the hydrocarbon compounds are left in the acid; is that the reason?

A. I so consider it.

Q. And those must be eliminated?

A. Practically completely.

Q. And if you do not eliminate them it will not work?

A. It will not work satisfactorily.

Q. I am not asking about that. Will it work in a commercially satisfactorily manner for the refining of such products as gasoline?

The Master: You were referring only to your laboratory practice and not to any commercial practice that you had, were you not, doctor?

The Witness: That is true, but it proves the point, which would apply to commercial practice.

Q. In other words, your idea is that in commercial operations you cannot refine with acid merely concentrated up to 75 per cent and still containing the hydrocarbon compounds from the preceding treatment?

A. I said nothing of the kind.

Q. What is your position?

A. My position is if you concentrate leaving in the hydrocarbon compounds, using the modern schemes of concentration, not the old fashioned ones, that that will be true. There is a very distinct difference between them.

Q. Did you say yesterday in the matter to which I directed your attention that you limit this to any particular method of concentration?

A. I think if you will look at the context, particularly the beginning of it, you will find that I did; and that I men- [fol. 1415] tioned the Chemical Construction Company. In

fact I have a definite recollection that I did refer to their apparatus.

Q. The question that you were asked is on page 9447 of the typewritten transcript of record, and refers back to Mr. Wilson's testimony to the effect that one of the advantages of using dilute sulphuric acid was that you did not have to spend so much money in concentrating. Now that is a fact, isn't it?

A. Yes, that was the reference.

Q. Now that is a fact, isn't it?

A. Yes.

Q. That is what this was all about?

A. And, incidentally, I can call your attention to that reference to the Chemical Construction Company. It is page 9449.

The Master: Dr. McKee, Counsel have a right to examine you in their own way. They have a right to get direct answers from you, and if there are any explanations that you want to make, after he gets through examining you your counsel are competent to bring those out. We will save time here if you will just answer the questions directly and not undertake to get away from the question that is asked.

Q. You understood yesterday when you testified, did you not, that the subject under consideration was the correctness of Mr. Wilson's statement to which I have called your attention?

A. That statement being what?

Q. That one of the advantages of using dilute acid was that it did not cost so much to recover it as to recover 93 per cent acid?

A. That is correct.

Q. You fully understand that that was what was being inquired about?

A. Yes.

Q. I just want to know now whether you still stand on the proposition that the thing Mr. Wilson said, could not be done. Is that true—do you still think that you told the Court the truth yesterday or not?

A. I still think, and I am quite certain I can show you if you will give me an opportunity, that it is cheaper to concentrate to 93 by the new methods than it is——

The Master: That is not the question at all, Mr. McKee.

Mr. Wiles: No, that is not the question.

The Master: Answer the question, Doctor.

Q. Are you standing on the proposition that the thing Mr. Wilson said it was cheaper to do cannot be done at all?

A. No, I stand on the proposition that it is cheaper to——

Q. That is enough, that is enough. You withdraw, then, any inference there may have been in your testimony?

A. No, not in the slightest. I did not say that.

Q. At the bottom of page 9374, referring to condensing under pressure, you said in the last answer on the typewritten page, "Yes, it increases the light ends of the gasoline and makes it very volatile, just as Lang says." I am directing your attention now to just the last clause "Just as Lang says." Will you please point out to me the language of Lang where he says the thing you quote him as saying?

A. It will be noticed in the first place that I did not quote Lang, but he gives it in Exhibit 135.

Q. Just read the language, please.

A. Exhibit 135, page 1675, second paragraph:

"Although it is my purpose to provide an apparatus of the above entitled character, the special object of my invention is to provide an apparatus embodying means of effecting the destructive distillation of crude hydrocarbon oil for obtaining as an ultimate product a hydrocarbon oil of predetermined volatility, and especially an oil of high volatility, or such high volatility that the oily will continue to vaporize at a relatively low temperature or that vaporization will continue when the oil is contained in snow, and the vapor therefrom burned for melting the snow."

Q. Is that the only basis you have for the statement that Lang says that pressure condensation increased the volatility?

A. No, it is not the only——

[fol. 1416] Q. Where does he say that condensing under pressure increases the volatility, as you said he said yesterday?

A. The whole patent covers that question of making a highly volatile product, of which his condensation under pressure is an important step.

Q. Does he mention any function whatever of condensing under pressure?

A. The patent is primarily for an apparatus, but it is——

Q. Does he mention any function whatever of condensing under pressure?

A. Yes. Page 4, line 21: "The valve 47 is so set as to allow the gas to escape through the valve and the pipe 48 connecting with it when the pressure of gas within the system reached a determined degree of compression above the pressure normally maintained therein."

Q. And you consider that a statement of the function of condensing under pressure?

A. What do you mean by "function" as used in your question?

Q. I am going to your testimony of yesterday where you said that Lang said pressure condensation increases the light ends of the gasoline and makes it more volatile, and I am trying to see if you can find one word in this patent to support that statement of yours. I want to give you every chance in the world to find it if it is there.

A. You will note that I did not quote it, but what I have said is that he speaks here of the fact that this is a process for making under pressure a product of such high volatility——

[fol. 1417] Q. Just a minute, that is not answering the question. I want you to find, if it is there, a statement by Lang which you said he made, that pressure condensation increases the volatility of the gasoline. Now, can you find it or not.

A. I did not say that he made it in that many words. I did not quote him at all.

Q. You said he said it, did you not?

A. But I did not say that he said it in those words? I said that he said it and he does in the entire patent here; and as I have said here, by this compression——

Q. Just a minute, you say he says it. You still say he says it, do you?

A. Yes, the whole patent says it.

Q. Will you call my attention to one single word in the

patent that supports that statement, the page and the line, please?

A. Page 1675, Exhibit No. 135, page 1.

Q. What line?

A. Line 19.

"An oil of relatively high volatility, or such high volatility that the oil will continue to vaporize at a relatively low temperature, or that vaporization will continue when the oil is contained in snow, and the vapor therefrom burned for melting the snow, in accordance with the process described in my patent—" and so forth.

And then again on page 1678, as to how he obtains this——

Q. Obtains what?

A. At page 1678, Exhibit No. 135, lines 21 to 29.

Q. Quote it, please.

A. "The valve 47 is so set as to allow the gas to escape through the valve and the pipe 48 connected with it when the pressure of the gas within the system reaches a determinate degree of compression above the pressure normally maintained therein. The gas so escaping may be saved or it may be conveyed back into the body of the furnace and burned."

Q. Do you want to add any other matter from the Lang patent as supporting the statement that you made yesterday as to its contents?

A. The whole Lang patent is——

Q. Any more matter. Do you want to add any other citations to the Lang patent as supporting the matter that you said was there yesterday?

A. The whole Lang patent——

Q. Now just a minute.

[fol. 1418] A. (continuing:) Is the matter that I wish to add in that case; because it is for an apparatus for making such a volatile product under compression.

Q. I direct your attention to the bottom of page 9446, where first your attention was called to an article of yours in "Canadian Chemistry and Metallurgy" for January, 1923, stating that Scotch shale gasoline is strongly odorous and somewhat colored and cannot be stored for any

considerable time without loss in quality. You were then asked whether Scotch shale gasolines are different in this respect from modern cracked gasolines, and you said, "No, they are substantially the same."

Having directed your attention to that matter I want to ask you first whether you meant to imply that modern American cracked gasolines cannot be stored for any considerable time without loss in quality; and you may answer that both with reference to 100 per cent cracked gasoline and about 50-50 blends with straight run.

A. In this connection, a year ago I——

Q. Just a minute. That is not what I asked. I asked you whether you intended to say yesterday that American cracked gasolines do go off.

A. They do.

Q. When stored?

A. They do. I have stored them and I know.

Q. Now, does that apply both to commercial fifty-fifty, approximately, blends, with virgin gasoline, as well as a hundred per cent cracked products?

A. If the commercial products on the market are fifty-fifty—I don't know their exact per cent—it does apply.

Q. Now, you say that you have stored these products—of whose manufacture?

A. A little over a year ago I purchased——

Q. Just answer the question—of whose manufacture?

A. Standard Oil of New Jersey. I cannot give you the list of them here, but there were ten of them, I think, purchased in and around greater New York.

Q. And in what quantity did you store them?

A. Small quantities.

Q. And in what?

A. Glass.

Q. White glass?

A. Yes, sir.

Q. Bottles?

[fol. 1419] A. Yes, sir.

Q. In the laboratory?

A. No, in a residence, in the attic. I say—I didn't store them in a residence—I had them stored.

Q. And what do you know, if anything, about the length of time that the commercial products are actually stored in the refineries, in regular practice?

A. I can't answer from anything excepting what I have seen in the printed publications on that. It is customary to store—or, gasoline is often stored for a considerable time—for example, they are now making gasoline, in December, which will not be used until next summer.

Q. Yes. It is the common practice to store enormous quantities of gasoline for anywhere from six months to a year and a half, is it not?

A. I imagine so—whether it is re-treated, before being sold, finally, I don't know.

Q. Have you any reason to suppose that is is?

A. Yes, I think that most of it is stored—that is, much of it is stored in the form of raw material, rather than in the form of a finished product.

Q. Do you know that it is the common practice to store enormous quantities of finished gasoline for anywhere from six months to a year and a half, and to sell it on the market, direct from storage?—don't you know that?

A. I would judge so, from some of the gasoline that I have seen.

Q. And it meets specifications, when it comes out of storage, doesn't it?

A. Not always, by a great deal—not Navy specifications.

Q. Whose gasoline do you know that does not now meet Navy specifications? I mean, by that, what is called New Navy—the U. S. motor gasoline.

A. What is your difference between the New and the Old Navy?

Mr. Wiles: I will not withdraw the question, your Honor. Will the Record show that the witness hasn't time to answer it?

The Master: The Record will show that the Master said to proceed with the cross-examination. The question is not answered.

Q. Will now call your attention to your testimony at the lower part of page 9387 of the Typewritten Transcript, where you said none of the plants—referring to cracking plants—either the old ones, or the present ones, can make one product only—they must make the two products.

Now, by the two products you meant gasoline and kerosene, did you not?

A. Yes.

Q. And did you intend to tell the Court, as a fact, that the modern cracking plants cannot make gasoline only without making kerosene?

A. That is correct—unless they are simply cracking kerosene.

Q. Do you mean to tell the Court, as a fact, that the modern cracking plant—say the Holmes-Manley—cannot take a gasoline stock and make out of it only gasoline, and tar, of course?

A. It makes——

Q. Now, listen—did you mean to say that to the Court?

A. Yes, though I want to add an explanation.

Q. Now, when you made that statement—because I thought that that was what you meant to say in your testimony—did you know, of your own knowledge, that it was true?

A. Yes—any cracking process that cracks gas oil must, at that time, make kerosene as well as gasoline.

Q. That isn't what I asked you. I asked you particularly, if you meant what I thought you meant—that the modern plant cannot produce only gasoline, and you said that was what you did mean. Now, I ask you whether, when you told the Court that in your testimony, you knew of your own knowledge that it was true—did you know that for the truth?

A. Yes—they need not necessarily——

Q. That is all. You say yes. You know that was true?

A. Yes.

[fol. 1420] The liquid drawn from 7 in the Barbet patent which is the heavy bottom of the pressure tar, goes to the paraffin presses, or to the furnace, to be burned.

Q. Now, did you intend, in your testimony, when you went into the question of exports of crude oil, to imply that in your opinion this Barbet process ran on American crude oil—was that the purport of your testimony?

A. Not quite. The purport of my testimony was that while it might have been on shale oil, it was more likely to be on the American petroleum, and it could be on either, because the—and what I was meeting, was the implication which had been made in the defense's presentation, that shale oil was the only thing available in France, whereas, actually, there were at the time at least twenty-five times

as many gallons of crude petroleum, as of shale oil, available—and there might have been still more.

Q. Now, I just want to get at the facts. It is your contention that this describes a process which may be a process of cracking, or handling and treating American petroleum—is that right?

A. Yes, it may be.

Q. And the process is one which fairly describes and fits a process of handling American crude oil?

A. Moderately well, yes, as is shown in the author's opinion, by the fact that he took out an American patent corresponding to this French patent.

Q. Now, will you tell me, please, what American crude oil there is—any product, or the residuum from pressure tar, which you can run through paraffin presses.

A. It would depend—I would say that you could do that with such a petroleum as a Pennsylvania petroleum, provided you didn't crack it down too far—that is, cracked roughly, only the gas oil portion, and didn't go for the——

Q. Don't you know that when you put in a mixture you will crack the heavy ends more than your light ends?

A. The defense expert has stated that the gas oil cracks easier than the portions above, or below, and I think that that is correct—I think his statement is correct—so that the heavy ends would not crack easier than the light ends.

Q. And so you contend, do you, as a purported expert, that you can put the heavy ends of a Pennsylvania crude through paraffin presses, even with no cracking?

A. I am not familiar enough to say, definitely, but it is my opinion that you can, and get paraffin out of it.

Q. Don't you know enough about this industry to know that petroleum will not press—that it will clog the pores in the filter?

A. I don't know whether that is true, or not. I know that it is true to some extent, in some mixed—

Q. Now, you know, don't you——

The Master: That is a complete answer to your question—"I don't know whether it will or not."

Q. And you do know that they do remove it by settling, which is a very expensive and slow process, don't you?

A. I know that that is one of the processes that is used; and that it is not particularly expensive.

Q. Now, another question on Barbet. You know that in ordinary running for steam refined, such as you have here described, you get off gas oil, and then you get off paraffin distillate, and in your bottoms you have petroleum—vaseline constituents—along with your cylinder stocks, don't you?

A. Yes.

Q. So that after your gas oil is off you get off the whole paraffin distillate cut, and you still have this heavy bottom remaining?

A. Yes.

Q. And, of course, you understand that petroleum is a [fol. 1421] mere scientific name for what is commonly called vaseline—the trade product?

A. I understand that vaseline is a copyrighted name.

Q. Yes, for petrolatum.

A. No, I think petrolatum was a later term.

Q. Well, it is the same stuff,—you know that.

A. Yes, it is the same material.

Q. And of course, if you were going to get off any petrolatum as a distillate, you would have to go to still higher temperatures, wouldn't you?

A. Than what?

Q. Than the temperature that prevails in the still after you have gotten off both gas oil and then the paraffin distillate stuff?

A. Yes.

Q. And the body of oil that was hot enough to distill off petrolatum, or vaseline, would be so hot that there could not be any gas oil in it, at all, except in mere traces—isn't that true?

A. Yes, in the ordinary steam distillation.

Q. Or in any other—at points away above gas oil?

A. No. You would have something of the type of a continuous distillation, where you are running in gas oil, continuously, using it in place of your steam. It would not, of course—

Q. Well, it would come right out again, because the oil body would be extremely hot—much hotter than the boiling point of gas oil, wouldn't it?

A. As I understand it, this is a steam distillation, rather than a dry distillation.

Q. It makes no difference, except to affect relative temperatures, does it?

A. Yes, it does make some difference—that is, you can carry out a steam distillation with almost no change of temperature, by using more steam.

Q. Of course, but the boiling point of petrolatum is very far above gas oil, isn't it?

A. On a dry distillation, yes.

Q. Surely. So, when a still is hot enough to be, under any condition, distilling vaseline, of course no reasonable percentage of gas oil could remain in it—isn't that true?

A. Not unless it was being supplied fairly continuously to it.

[fol. 1422] Q. And if it was it would go in and come right out, because of the immense temperature that would exist there.

A. No, not necessarily.

Q. How long do you suppose you could hold gas oil in a still 200 or 300 degrees hotter than the boiling point of gas oil?

A. If it was mixed with the other material you would hold a fair proportion.

Q. Just a little. How much?

A. It would depend upon your conditions, on what your residue was, as well as on what kind of gas oil you were using.

Q. And it would be a very small percentage that could stay in, at all, wouldn't it?

A. I don't think it would be a very small percentage.

Q. Do you suppose you could hold five per cent of gas oil in a body of oil two or three hundred degrees above its boiling point?

A. Certainly—even several times that. We do do that in the case of kerosene in gas oil—just exactly that same thing.

Q. You made several statements about the heat of cracking yesterday, and the day before. How much is it—to crack, we will say, to make 30 or 33 per cent of modern gasoline—such cracking as occurs in a Burton still—what is the heat of cracking per pound of the material treated?

A. Cross, I think, gives that as a hundred degrees, B. T. U., per pound. Leslie, in an article which gives probably the most accurate determination that has been made, in I think it is the September or August issue of Industrial and Engineering Chemistry, gives it as a figure slightly different. I am not quite certain as to the figure.

Q. And that is a hundred B. T. U.'s per pound of the products—of the gasoline—is it?

A. I think not. I think it is a hundred B. T. U.'s per pound of oil cracked.

Q. Do you know? I want your knowledge on this. You are an expert, and you ought to know some of these things. There is a difference of 200 per cent. Now, which is it?

A. The difference would be only a few per cent. That is, of the portion cracked. A hundred B. T. U.'s per pound, there—you would get some carbon and some gas, and the difference wouldn't be so great.

Q. It is a hundred B. T. U.'s per pound of what? Of the gasoline produced? When you are getting thirty or [fol. 1423] thirty-five per cent of gasoline is it a hundred B. T. U.'s per pound of charging stock, or a hundred B. T. U.'s of the gasoline produced?

A. It is neither one. I think that it is a hundred B. T. U.'s of the portion of the oil which was cracked, which would make it almost the same, though not quite—the same as a hundred B. T. U.'s of the gasoline produced.

Q. All right. Then, in one of the commercial processes, such as the defendant use, it is about thirty-five B. T. U.'s per pound of the charging stock?

A. I think so. Now, I think that is the figure that Cross gives. Leslie's figure is somewhat different.

Q. Well, that is as near right as you know.

A. No, I think Leslie's figure is more accurate.

Q. What is his figure?

A. I am not quite certain, but it is not far from the same figure.

The Master: If you don't know anything about it, of your own personal knowledge, Doctor, how do you know which is the more correct—Leslie's or Cross's?

The Witness: I went over it last summer and decided that the evidence was a great deal more in favor of Leslie, but the actual numerical figure I don't recall, precisely.

Q. Now, we can accept 35 per cent as your statement, can we?

A. It will serve, although that is not my best judgment.

Q. Now, you spoke of the latent heat in processes of this kind. What is the latent heat of evaporation, at the temperatures and pressures in these pressure stills—roughly?

A. Do you refer to gasoline, or kerosene, or gas oil—or what?

Q. The pressure distillate that is evolved in the stills—what we have to put in—I am trying to get at the heat input.

A. It is a figure of the same order, but slightly less, per pound of the product.

Q. So that the total of the two would be less than required to raise the material a hundred degrees.

A. Not by a good deal—for the specific heat of the oil is very much less than that of water. You are thinking of water.

Q. No, I am not. What is the specific heat of oil, at high temperature?

[fol. 1424] A. It is low but I don't recall the figure.

Q. But the total B. T. U.'s is on-y sixty, on your figures, or less—the total B. T. U.'s required, is only 60, or less, for the conversion and evaporation?

A. As I said, I don't recall the exact figures. You are giving a figure of 25.

Q. You said it was less—that is 60 or 65, is about right, isn't it?

A. It is about right, yes.

Q. Now, if the specific heat of oil is around three-fourths, and I think at the higher temperatures it is still perature?

A. I don't think so. I think it is much lower.

Q. Will you give us the best figures you have, on that?

A. The average between ordinary temperature and your high temperatures, runs to about six-tenths, not three-fourths, and I think at the higher temperatures it is still less.

Q. Doesn't it increase rapidly, in fact, as you go up?

A. That is not my recollection. I am not certain of those numerical figures.

Q. But, in any event, fiuring it any way you choose, you could not be far from the fact that the heat of evaporation and chemical transformation, answers to about a hundred degrees, or a little more—heating of this oil—around 125, maybe, or somewhere in that order?

A. If you say so I will take your word for it. I don't know.

Q. All right. But you made the statement somewhere—on page 9276, of the Typewritten Transcript, to be quite accurate,—and I think you were speaking of the Pielsticker patent—that there is very little rise in temperature after cracking starts, in one of these processes. Now, just how broadly did you mean that statement?

A. I was thinking, in this case—I am not quite certain that I understand the meaning of your word "broadly." Do you mean the difference in temperatures, or——

Q. You were asked a question about Mr. Wilson's testimony on Pielsticker, where he said there would be equal rise in temperature—that there was substantially an equal risei n temperature, and you said that must be wrong, because once your cracking is started you get relatively litt-e rise in temperature. Now, you made that statement in the form of a general truth, and I wanted to know if that was what you meant.

A. I think that it is generally true that——

[fol. 1425] Q. Well, that is all.

A. (continued)—with processes of this type.

Q. Now, cracking starts somewhere around 700, does it not?

A. That has been the statement here, but actually the experiments in the literature show that it begins actually around 570, and——

Q. Well, I will not debate that question. We will say it starts at 570, and the cracking processes of the art run all the way from figures around that, up to at least 900—don't they?

A. Yes.

Q. Consequently the temperature must continue to rise right along, from the time cracking starts, in some of those processes?

A. That is true, but those processes are not of this type, as I limited my answer, a moment ago.

The Master: What type?

The Witness: In the type of Pielsticker.

Q. Well, you can raise the temperature as far as you want to, can't you?

A. Yes. We have vapor phase processes that are running up to even 1700 degrees.

Q. Yes, but suppose the process only runs up to what you have stated to be the critical temperature—we have processes of that kind, haven't we? Like the Holmes-Manley?

A. Yes.

Q. Where the stuff stays liquid all the time, according to your statement.

A. It certainly does not stay liquid in the Holmes-Manley process, in the end of the tubes.

Q. Well, you can take a Holmes-Manley and run it to any out temperature you please, can't you, and short of this critical temperature that you are talking about, regardless of the charging stock?

A. Yes, but you are getting cracking taking place, so that you are getting vaporization on there, and you are heating your vapors in part.

Q. That is all right—I am not arguing about that. You can take a process and begin your cracking—if you say that that is the figure, I shall not argue about the difference here—570—you can raise it up to 820, and it will warm up all the time, and still be principally liquid, can't you?

A. Yes, but the cracking only just begins and goes [fol. 1426] slowly. The experiments, in the beginning, there, were not a commercial rate.

Q. But you can heat it just as far as you please, up to 820, anyhow, can't you? I don't care what the temperature figure is—I want to find out the temperature to which you can heat it.

A. You can heat it there.

Q. Yes, and you can heat perfectly, regularly and smoothly, up to say 820?

A. No. Cracking has begun, and if you will give it time it will keep your curve—your curve will come off, and in any case, unless you heat it very quickly, you can't even heat to that temperature.

Q. But you can heat—if you run your fires right, to make the temperature rise continuously from 570 up to 820, can you not—and never have it stop rising?

A. Yes, if you distill off part of your portions that you have formed by cracking.

Q. Surely. Well, that is what would happen in a Pielsticker still—it would distill off, wouldn't it?

A. Yes, in part—just as in Burton.

Q. Well, in whatever forms, it would distill right off, wouldn't it?

A. Most of it—a portion of it would.

Q. So that there is no reason in the world why Pielsticker, if you regulate your fires right—why you can't run right up from 570 to 820, without stopping your rise, at all?

A. But Pielsticker says differently.

Q. You can do that, can't you?

A. Yes, but not operating as Pielsticker says.

Q. Please answer the question. You can do it, can you?

A. In the Pielsticker apparatus, yes—if you want to carry it out that way.

Q. Certainly. Now, your proposition on Pielsticker, as I understand it, is that because there is some carbon in the tubes, therefore you have to go to at least around 700 in the tubes. That is correct, isn't it?

A. I wouldn't limit it even to 700.

Q. Well, it is 700, or higher, in the tubes.

A. Yes.

Q. That is your proposition?

A. Yes.

Q. And you could not get the carbon there, unless you did.

[fol. 1427] A. Oh, you might get traces, but not in an amount which would be bothersome.

Q. Of course, once you start to get carbon it accumulates very fast; by oil catching in the interstices, and getting more and more—that is true, isn't it?

A. It does accumulate, unless the current of oil is fast enough to wash it away.

Q. Once it sticks on the bottom it piles right up from then on?

A. Yes, unless it is washed away by the current of oil.

Q. And it will do it in a coil, just as well as anywhere else, if it gets started?

A. In course of time.

Q. Now, Pielsticker describes a very large number—or a considerable number of varient processes, does he not?

A. He describes a considerable number of processes, but I don't understand that they are exclusively varients of one process.

Q. There are a lot of different processes described there?

A. Yes, sir.

Q. And in one of those processes he runs his still on emulsified crude oil, doesn't he?

A. Yes.

Q. Now, you seemed to intimate, yesterday,—or, at least, I thought you did—that the water is emulsified crude always settles out if you give it a chance.

A. No, no.

Q. You didn't mean that, did you?

A. It don't, always.

Q. Some of them won't settle out at all and you have to break the emulsion some other way—either chemically, or by distillation, or electrically.

A. There are such schemes. There are still other schemes used.

Q. But the principal schemes in breaking emulsions, are distillation, Cottrell electrical precipitation and chemical emulsion breaking—are they not?

A. That is true in the United States, but not true in the rest of the world.

Q. Now, when the oil is emulsified it has practically always, salt water in it, hasn't it?

A. Practically always.

Q. And there are immense quantities of that out in California—emulsified oil, containing salt water?

[fol. 1428] A. I think so, as well as in the rest—in the mid-continent.

Q. And that is particularly true of, for instance, the Santa Maria field, is it not?—practically all of the Santa Maria field is badly emulsified?

A. I don't know.

Q. You find in the Dubbs' patent, offered by the plaintiff, here, a description of the Santa Maria crude oil, don't you?

A. I don't recall, but I can see.

Q. Well, it is true. Never mind. Do you know anything about running that kind of oil in a pipe still?

A. I have had no experience in running a pipe still.

Q. Now, don't you think it is reasonable to suppose that if you run such an oil, the water would boil off and the salt would settle out?

A. When do you mean?

Q. When you run it in almost any kind of a still—the water would boil off, leaving the solid salt, and the salt would settle out.

A. It would depend somewhat on your arrangement—your salt would separate as a solid, but whether it would settle, or whether it would emulsify, I don't know. It would depend upon your apparatus.

Q. In an ordinary pipe coil the salt would settle out, wouldn't it?

A. It might, or it might not. It would depend somewhat, on what the velocity of the oil through the pipe was—whether it would settle out—if, by settling out, you mean adhere to the pipes.

Q. Yes, and if it did settle out, of course the pipes would coke up very fast with the oily body, at a very low temperature.

A. On the supposition that it did settle out and adhere to the pipes.

Q. And in running such oil—emulsified oil—you might have your pipes very badly coked, without getting the temperature of the oil in the coils over 300 or 400—might you not?

A. On those suppositions, yes. Just a moment, I want to change that. If you mean by 300 or 400 the average temperature of the oil rather than the——

Q. I mean the out temperature, taken from the thermometer at the outlet end of the pipe.

A. I do not believe that those temperature—I think you [fol. 1429] would have to go somewhat higher than that, perhaps 600 or 700. Certainly not at 300 and probably not at 400.

Q. Have you ever heard, Dr. McKee, that a still of this kind was run in the presence of counsel and the Master in Chancery in the so-called Dubbs suit, for months, and

that I was there? You never heard that rumored around the oil industry?

A. I know nothing of that particular incident. I do know there is a Dubbs' suit, and some things about that; but not of that incident, if it existed.

Q. You do not believe it would be possible to get coke in a coil where the temperature of the outflowing oil never exceeded 400?

A. I don't know.

Q. You do not know it would not be true?

A. I do not know that, either.

Q. And it might be true if the salt settled out?

The Master: He said he does not know anything about it. What is the good of going any further.

Q. Yesterday afternoon—I cannot quote the page—you said that in 1910—I think this was in connection with Allen, but I may not be right—that cracking for kerosene was very uncommon and there would be very little cracked kerosene in market kerosene at that date. Is that what you meant to say? I have not the citation here, because I have not had time to look it up.

A. This, I think, was said in connection with Allen, but I called attention to the fact that Allen's publication was in 1910 and this was somewhat earlier and the period would be then somewhat earlier than this—say, five or seven years earlier.

Q. Your proposition is that at some indefinite time, five or six or seven years before there was very little cracked kerosene in ordinary kerosene, kerosene on the market?

A. On the market, that is correct.

Q. What percentage actually was present, do you know?

A. I would feel very certain that it was not more than a few per cent. Perhaps one or two per cent at that period of time.

Q. There was more than there is today, was there not, a greater proportion?

A. No, I think not. I think there is more today due to its being a by-product from the cracking of gasoline.

Q. I will now take so-called straight run kerosene from [fol. 1430] the ordinary coking stills. There was certainly more in those days than there is today, because they wanted to make more kerosene.

A. I think not. I thought I had the figures here. I have part of the census figures here, but they do not give me the data that I sought. I think we are producing more kerosene, practically every year, than we were the year before.

Q. I mean the percentage of cracked kerosene as compared with—do you know what I mean by virgin kerosene?

A. I think I do.

Q. All right. The percentage of cracked kerosene as against virgin kerosene from the coking stills in 1905, we will say, was certainly greater than it is today, wasn't it?

A. From the coking stills.

Q. Yes.

A. But not on the market.

Q. I am not talking about what goes back from the pressure stills, because the only process that they then used to any considerable extent was the coking still process, was it not, in running kerosene?

A. A good deal of it was run by a coking still, but even more was run where you stopped when you got your kerosene and did nothing more—which would be virgin kerosene.

Q. Yes, that would all be virgin.

A. It was from the same coking still, though.

Q. And that was very extensively practiced by the large refiners in 1905, was it not?

A. Yes.

Q. Because even at that day they were not wasting their heavy ends, they were getting their lubricants out, were they not?

A. Yes.

Q. And their paraffin wax?

A. Yes.

Q. So the big refiners were even then principally using the coking still just as they are now.

A. But they would not be cracking for the kerosene in getting their paraffin wax products.

Q. In the ordinary coking still at the ends of the run you cracked extensively during the taking off of the paraffin distillate and the slop and the still wax, do you not?

A. You do some cracking.

Q. Yes.

A. I would not say extensive.

[fol. 1431] Q. Well, you do crack during that part of the operation?

A. As little as possible.

Q. And that kerosene that you make by cracking at that time goes into the regular straight-run kerosene, does it not, and always did?

A. I do not know about that, whether it always did or not. I do not think it did.

Q. Wasn't the enhancing of the kerosene yield by that method the very purpose of the still-heads and run-back devices that were employed in those days extensively, concerning which you have read from Aisinman and Leet and others?

A. That is not of that period, if you remember.

Q. I know, but that process continued right up to date, did it not, with even better towers on?

A. Atmospheric cracking has not increased, to amount to anything.

Q. We are running more crude than we ever ran before, are we not?

A. I know, but not for the purpose of cracking.

Q. But we are running seventy-five per cent of our crude, are we not, in the big plants—Mid-Continent Crude; at least through just that kind of stills, all the time, and cracking?

A. With as little cracking as possible.

Q. You have got to crack, have you not, when you go down to coke; you have got to crack, you cannot help cracking?

A. Oh, yes; that is correct.

Q. And we crack a lot at the end, do we not? We have to.

A. Yes, but that cracked kerosene that you get there largely goes in as a stock for recracking to make gasoline, such as in your continuous still at Whiting or in the stills throughout the country.

Q. Dr. McKee, do you really know the practice of a refinery in this matter; do you know how much is in these different cuts—really know it yourself like a refiner?

A. I know to some extent. I do not attempt to carry in mind any large number of figures through there.

Q. At the crude stills, doesn't all the kerosene that is cut go into one set of tanks in the ordinary refinery?

A. I do not think so. Not in the ordinary refinery. It may in some of them, in others it does not.

Q. You think it is segregated between the cracked and the uncracked in any refinery of any size?

A. Yes.

Q. Who segregates it?

[fol. 1432] A. American Refining Company does for one.

Q. I mean the cracked kerosene at the crude stills?

A. I think they put that in along with their Dubbs kerosene and use it as cracking stock through a Cross process.

Q. You do not suppose that was done in 1905, when there was a market for kerosene?

A. But you are asking about the present-day practice.

Q. All right; that is all right. If you just take an ordinary coking still run on an ordinary average Mid-Continent crude, how much kerosene will you get off up to, say, 650?

A. It is pretty hard for me to know what you mean by an average Mid-Continent crude, whether you mean an average of the crudes, or whether you mean what a particular refinery obtains.

Q. I mean a non-premium Mid-Continent crude.

A. A non-what?

Q. A non-premium Mid-Continent crude; just an ordinary Mid-Continent crude. Oklahoma, say. You might, while you are looking this up, add this other thing—find out how much you get off up to 650, and then find out how much kerosene you get off above 650, if you can find out, and if you do not know, why, say so. I am talking now of still temperatures, understand.

A. The figures given here, which are apparently from government reports, give as the portion which was then considered as essentially the kerosene cut for Mid-Continent products all the way from twenty to forty per cent, generally around 35 or 36 per cent, as the portion boiling according to an Engler test in the range of kerosene.

Q. And that would be the part that would really be virgin?

A. Yes.

Q. Do you not know as a matter of fact, in the ordinary coking still you supplement that by nearly fifty per cent, or forty per cent at least, or more, that is cracked, made by cracking, and that comes off at the time you are getting

off your gas oil and paraffin distillate and still wax and slop?

A. I know that some supplement it, but I do not know the figure. I know that others do not.

Q. You were asked some questions about Benton. Benton says, does he not, in substance, that he transforms the entire crude oil into a homogenous illuminating oil?

A. Do you recall where he says that? I do not recall quite that kind of statement. The exhibit number is 150.

[fol. 1433] Q. Lines 20 to 30 is good enough. That is the substance, if you care to read it.

A. That has not that meaning, because in the next column he defines his homogenous vapor there quite differently—defines that ferm. In the next column, 93, he says: "The different hydrocarbons will vaporize simultaneously forming a homogeneous vapor instead of first vaporizing the benzine and then the next heaviest hydrocarbon as heretofore mentioned."

Q. Exactly, and he says the whole thing will be an homegeneous illuminating oil, does he not?

A. He says he will produce a homogeneous illuminating oil.

Q. Yes, without using the benzine which is heretofore lost?

A. I do not see that statement, but I think he does make a statement of something of that character.

Q. And he says it will be "a homogeneous fluid possessing all the most desirable properties of a high test illuminating oil and containing in its homogeneous bulk substantially all of the benzine?"

A. I find that.

Q. That is on page 1824.

A. I see it there, thank you.

Q. High tests, of course, mean over 100, does it not?

A. Over 100 flash point.

Q. Yes, over 100 flash—probably around 110 or 115. Nowadays we will call it 150 if it was a high flash, wouldn't we?

A. Yes, but not in that day.

Q. But in his day it would mean 110?

A. Yes, or at least 100.

Q. He talks about an oil that has in it originally 12 to

15 per cent of benzine, doesn't he—crude oil with 12 to 15 per cent of benzine?

A. Can you call my attention to the line where he gives that percentage? I am not able to locate it.

Q. It is 12 per cent. I think I was wrong on the 15. It is line 65, page 1. As high as 12 per cent.

A. Yes.

Q. So that the purported operation of the process is to take an oil containing 12 per cent of benzine—which would have a flash far below zero, would it not?

A. Yes. That is, the benzine would have.

Q. Well, the crude oil itself would have?

[fol. 1434] A. I don't believe it would at a twelve per cent have below zero Fahrenheit.

Q. I do not want—if you will look at that report, you will find every one of those California crudes with as much as one per cent had a test around zero. Look at Bacon and Hamor and see if I am not right. California crudes—which gives the flash on the crude.

A. And the Los Angeles one has a flash; the first one given, a flash point of 107 degrees Centigrade which would be about 220 degrees Fahrenheit.

Q. No, but look at the ones that have any appreciable gasoline in them. 1.3 per cent gasoline flashes below zero. The one you cited had no gasoline.

A. Well, I took the first one that is given.

Q. Then slide down to one of those I have indicated.

A. 1.8 has minus 1 degree Centigrade, which would be—

Q. 32.

A. Which would be 30.

Q. 30, yes.

A. Thirty degrees Fahrenheit. And some of the others there—two per cent has minus two.

Q. Yes, and you know from that. Some of them have minus 12.

A. Only one has minus 12.

Q. And that has 1.3 per cent benzine in it?

A. Yes.

Q. So that you know from comparing those figures that a crude oil with 12 per cent of benzine in it is going to flash below zero Fahrenheit, isn't it?

A. That would be thirty degrees lower still than the 1.

Q. No, minus 12 is pretty near down to zero.

A. And 1.8 is up at 30 Fahrenheit; 1.2 is 15 degrees above.

Q. Don't you know as a purported expert that if you have 12 per cent benzine in a crude oil you are going to get a flash below zero?

A. I would not be certain that I would.

Q. Pretty close to zero, anyway?

A. I think you would get it below zero Centigrade, but not below zero Fahrenheit.

Q. Anyway, he takes this oil according to his process and he transforms it into an oil whose boiling point is going to be around 250, 275, and maybe 300.

The Master: What is the question. That is a statement.

Q. (Continuing:) Isn't that a fact?

[fol. 1435] A. That would be his upper——

Q. No, that is his initial.

A. No. I do not think it would be as high as that.

Q. It has 110 flash when he gets through with it. You are going up 150 degrees anyhow, are you not, or around that for your boiling point?

A. I would expect if it was high test illuminating oil that his initial boiling point at that date would be in the range of 220, 250, rather than 300.

Q. All right, 220, you say?

A. Yes.

Q. All right. And that is what he says he does, isn't it? You say if he does it. That is what he says he does?

A. That is the statement in this citation.

Q. Exactly. And also the heavy ends of his oil have got to be made into the same material, of course. That is true, too, isn't it?

A. If we accept his——

Q. If we accept the fact that he did what he said he did, then what happens in his process is that he cracks the heavy fraction down to kerosene and he synthesizes the gasoline up to kerosene and the whole product when he gets through is a product with an initial of around 220 or 250, according to your statement?

A. I do not interpret the patents, but on the suppositions that you have made from time to time—and I have answered you on those suppositions—it is correct. But——

Q. If he does what he says he does, that is what he has to do, is it not?

A. I do not think that even he thought there would be no by-products here.

Q. I am not interested in that. I asked you whether he does——

A. No one skilled in the——

Q. If Benton does what he plainly says he does, then he synthesizes the gasoline into a heavier product and cracks the heavy fractions into a light product, does he not—that is, both going to the kerosene range?

A. Benton does not say quite that. He says he cracks his light product and gets a heavy product, and he cracks a part of it, he does not say he cracks all of it, and gets a light product.

The Master: The answer is not responsive. He did not ask you what Benton says; he says if Benton said that, isn't that what he has to do.

The Witness: If Benton said it, yes.

Q. You produced from Bacon and Hamor a number of figures on the unsaturated contents, which you said showed the unsaturation of the distillable portions of American crude oil. I am correct in my understanding that that is what those figures that you gave were given to show, am I not?

A. Not quite. They are the percentages of the crude oil, and, incidentally, I gave the percentage of the undistillable portion so that we would want to get the percentage of the distillable portion, subtract the percentage of asphalt, and divide it into the percentage of unsaturated hydrocarbons.

Q. No one of those tests that you quoted is a test on the distillable portion and only on the distillable portion of any American crude, is it?

A. Yes.

Q. Which one?

A. There were some which had no asphalt, and I mentioned them, I think, at the time. Those would be on the

distillable portion, without having to go through a calculation.

Q. Do you know that any one of those crudes was actually distilled over and the test applied to the distillable portion?

A. I did not see it done. I cannot say from personal knowledge, but as I interpret this, the analysis has been made here, and the analysis shows that the unsaturated hydrocarbons present——

Q. In the total crude, in every case, isn't it?

A. That is my understanding.

Q. Exactly.

A. But if the total crude carries no asphalt, then it is in the distillable portions.

Q. But wait a minute. Take an ordinary Pennsylvania crude. That contains no asphalt, does it?

A. Some carry asphalt.

[fol. 1436] Q. Yes, plenty of them do not, that is right, isn't it? Lots of them do not?

A. I think that is right.

Q. Most of them do not?

The Master: Well, if you do not know just say so. What do you say, Doctor?

The Witness: I think that that is true, but I am not certain.

The Master: All right, go on.

Q. Can you take any of those oils and distill them clear down to zero in a flask without cracking—any one of them?

A. No, but that is not the point of this—of this testimony.

Q. I know it is not; that is why I think your table hasn't anything to do with the proposition you cited it on. You cannot take an oil that has no asphalt and distill it all overhead, can you?

A. Yes, you can, and that is the way they determined their asphalt here, when they found there was no asphalt. They distilled it over, using steam. You are talking about a dry distillation.

Q. You are sure that is what they did?

A. Let me——

Q. Can you distill 100 per cent overhead with steam, with a vacuum, or both together—any kind of a crude that you ever saw, and tell me what kind, if it is so.

A. I would so understand, for example, the oil from Oklahoma given on page 162—that that was substantially true there. The amount of asphalt present was only fifteen one hundredths of one per cent, and that meant that all the rest of it had been distilled over.

Q. And that was all distilled over in the regular steam distillation method of determining asphalt, that is your understanding, is it?

A. Yes. That is 99.85 per cent of it was distilled.

Q. Was distilled over from an Oklahoma oil?

A. Yes.

Q. And you are seriously telling the court that there is an Olkahoma oil where you can do that; do you really think that is true, or do you want to look for some other explanation?

A. I understand that that is what was done at that time.

Q. And that is your representation to this court, that that is the fact, and could be the fact? I ask this on your [fol. 1437] qualifications. Don't you know enough about oil to know that that cannot be so?

A. No, I think that that——

Q. You think it is so, then?

A. I am inclined to think that the figures here are probably correct.

Q. And that that is the way that test was made? Now, don't look it up; you really honestly mean to say that you know so much about this industry that you will tell the court that?

A. I think that that is the way that test was made.

Q. And you believe that that result is possible with an Oklahoma oil?

A. Yes.

Q. Really believe it?

A. I think that that was the way it was made and that the result was obtained.

Q. And you honestly think such a result is possible?

A. Sure.

Q. The California oil tests were made that way, Doctor, I admit it. Look at the California oil asphalts, the first table that you have in that book, Table No. 28 on Page 131. You will notice that the asphalts determined in that manner are 48, 53, 42, etc., in the last column to the right. You notice that, do you?

A. Yes, sir, they are in——

Q. Just a minute. You see that that is correct, and I correctly stated the figures?

A. I think you have correctly stated the figures.

Q. Yes.

A. Those are, of course, heavy asphaltic oils.

Q. I know that, and they are in startling contrast with the figures for all the rest of the country, are they not?

A. No. On page 138 you have here an Indiana oil given as having 47 per cent asphalt.

Q. Sure, and no residue at all after kerosene is off, practically—I mean practically no kerosene cut?

A. In that particular one.

Q. Yes. There was not any light oil in it at all, it was pretty near an asphalt as it came out of the ground, was it not?

A. No, they simply have made no distillation in that case; there is no residuum, there is no test made of it.

Q. You know, as a matter of fact, the Gulf Coast crudes [fol. 1438] and Louisiana crudes, a lot of them, are pretty near as asphaltic as the California crudes; don't you know that?

A. No, not as the California crudes.

Q. You do not know that as a man purporting to be skilled in this art; you do not know that?

A. I answered your question.

Q. I wish you would turn, please, to page 126 of this Bacon and Hamor book which describes the method by which the asphalt was determined in that table, and just read it aloud on the record. You have said to the court that it was the residue that was undistillable. Now, you just read what it actually was.

A. The paragraph which you think is the way that it was determined, reads as follows:

"The asphalt was determined by Holde's method, by weighing off one gram of residuum and shaking this with 40 c. c. of gasoline which was free from unsaturated hydrocarbons and which boiled between 65 and 95 degrees C. After shaking this was allowed to stand for 48 hours and the precipitated asphalt was dissolved in benzol, dried at 105 degrees, and weighed. Accurately greater than one per cent was not claimed for these analyses."

Q. Having read that paragraph, you know, do you not, that the asphalt given in those asphalt tables was not the undistillable residue, but a very fraction thereof?

A. If it was made in this way.

Q. The book says it was made in that way, does it not? It is a government publication, isn't it?

A. I do not see that the book says it was made in this way. I have looked for it. I do not see that. I have been looking for it, but I do not see that it makes such a statement.

Q. Will you just go back to page 122 of the book where the subject of these tables begin, where it says, "The following tables, Nos. 27 and 29, with the exception of the analyses of the crude petroleum of the oil fields of California, Table No. 28, are based upon an extensive compilation given in Mineral Resources of the United States. That is a fact, isn't it—that appears there?

A. Yes.

Q. The book then proceeds with a general description of the methods by which the samples were collected for that and the next page, doesn't it?

A. Yes.

[fol. 1439] Q. And then on the next page, 124, starts a section called, "Method of Analysis" and that is the method that was employed in making up the tables, was it not?

A. Possibly, I don't know.

Q. Can it be anything else, occurring where it occurs?

A. They are giving here two methods, two sets of methods. I should expect——

Q. This first set was made—Tables 27 and 29, the book says, were tested by the first method of analysis, doesn't it? That is the method beginning on page 124 and running to the middle of page 126, is that correct?

A. Where does it say that?

Q. Beginning on page 122, "the following tables with the exception of the analyses of the crude petroleums of the oil fields of California are based upon extensive compilation given in Mineral Resources of the United States," etc.

A. But it does not say that that is the method of analysis.

Q. You think it is not?

A. I am not certain. I am inclined now to think that it is, though I——

Q. All right, we would have saved a lot of time if we got that first.

A. I am not certain about it at all. There is no real indication.

Q. There is not any other analysis, or any other thing there that that could apply to, this method of analysis, except those tables, is there?

A. Another method of analysis is given here on the next few pages just preceding the tables.

Q. And that is the one that says it was used, and it is headed "The Crude Petroleums of the Oil Fields of California," and the thing says that was done another way, does it not?

A. It does not say another way; it says it was carried out in that way for the oil fields of California.

Q. Yes, sure, and when you carry it out according to the California analysis, or the one that is entitled "The crude petroleums of the oil fields of California," you get asphalts around forty per cent, when you carry it out by the preceding analysis, you get them in a fraction of one per cent; that is a fact?

A. And up to forty seven per cent.

Q. Don't you know perfectly well that if the tables 27 and 29 were made by the method of analysis on page 124 to [fol. 1440] the middle of page 126, the asphalt reported is but a small fraction of the undistillable part?

A. No, I understand that the asphalt by the two methods would give results which are approximately agreeing, but that the one which is given here—it claims as to its accuracy, "Accuracy greater than one per cent was not claimed for these analyses," so far as the asphalt is concerned. In other words, you would get the same within one per cent.

Q. Don't you know, as a so-called expert in this art, that the Holde precipitation method does not come within several hundred or several thousand per cent of the distillation method of determining asphalt, and shows an entirely different thing?

The Master: Without consulting any books you ought to know that one way or the other.

A. I do not so know.

Q. Will you look at Table 29 and see the residuum remaining in these columns after 300 Centigrade, and see if

you do not want to modify your testimony that this asphalt figure purports to show the undistillable amount?

A. I do not so understand it, because there would be a great deal distilled above 300.

The Master: You do not want to modify your statement?

The Witness: I do not want to modify my statement except——

The Master: That is the answer to the question.

The Witness: Except within one per cent accuracy in the method of analysis.

Q. You know very well, do you not, that asphalt is normally divided by the art into two parts commonly called asphaltene and petrolene parts?

A. I know that there is such a division made.

Q. And, broadly speaking, those divisions are into the part soluble in naphtha and the part not soluble in naphtha.

A. I do not recall whether it is naphtha or not, but in a solvent.

Q. In a petroleum solvent, but not in carbon disulphide or carbon tetrachloride, or something of that sort?

A. I do not recall just which of the solvents is used, but it is a solvent differential.

Q. In this Holde method you take your oil, a gram of it, and you dilute it very heavily with naphtha, do you not?

A. A saturated naphtha.

Q. Yes, a saturated petroleum naphtha. That is your solvent. What happens then if that—a part of that—which is insoluble in naphtha falls to the bottom. That is the Holde test isn't it?

A. Yes, allowed to stand.

Q. And all that part of the asphalt which is soluble in naphtha remains in solution?

A. If there is any, yes.

Q. And there are very large parts of all asphalts that are soluble in naphtha, are there not?

A. I understood from the Holde method that it is accurate to one per cent as the statement here said.

Q. Will you just answer my questions. Aren't there very large percentages of asphalt, ordinary asphalt derived from petroleum by steam reduction, which are soluble in naphtha?

A. I don't know. I don't think that—I do not know on that, as to what per cent.

Q. It is a fact, isn't it——

A. Before we start, Mr. Wiles, I find——

Q. I don't want any volunteer testimony.

A. This is not testimony, at all, but I find that a wrong numerical value was used in some of yesterday's testimony. Do you want me to correct that now, or——

Q. You can correct it when you please. I am informed that my associates cannot find the Science article that you referred to—can you give us any better data about it?

[fol. 1441] A. I have since looked it up. It is Volume 35, page 388, of 1912—"Science."

Q. Now, it is a fact, isn't it, that in performing the sulphuric acid test for unsaturation, if the temperature rises you get a higher result?

A. Slightly, but only slightly.

Q. A rising temperature does give a higher result, does it?

A. Yes, if you have rise enough.

The Master: Is that the applied temperature, or is it the temperature of reaction?

Mr. Wiles: The reaction tends to raise the temperature, and thus increases its own heat, and it gets more violent if you let the temperature rise.

The Master: That is, the temperature of reaction rises?

Mr. Wiles: Yes. You start it cold and if you let it get hot it gets more violent.

The Master: I just wanted to know to which you referred.

Q. Now, referring to the test you made on benzol, your present testimony is to the effect that if you perform the standard test the temperature rises and you don't get freezing—is that right?

A. I don't think that is right. You do not get freezing, due to the fact, not of temperature rise, but due to the fact that there is some benzol sulphonic acid first formed, which dissolves in the benzol and lowers the freezing point below that of the temperature of the test—that is, zero degrees centigrade, i. e. 32 degrees Fahrenheit.

The Master: Then the standard test is not an accurate test?

The Witness: The standard test is accurate within a per cent or so. It doesn't pretend to be extremely accurate.

Q. Now, in the standard test you start cold, but you don't keep it in ice water during the test?

A. I think you do—at least, you have it cold, and keep it cold. You don't do it all the time, because we are later centrifuging, and in that case it is not kept cold.

Q. In the standard test, as prescribed, you start with your material cold, and put in the acid and shake it outside, in the air, and let it warm up as much as it naturally will, don't you?

[fol. 1442] A. As I recall, the standard test is somewhat different from what you say, though I haven't the details in mind now. It has been a year or more since I have looked that up—and that is, that you put in your sulphuric acid, chilling it down, and then put in your oil, and run your test. Perhaps I may be wrong as to which one is added.

The Master: There is some document giving what the standard test is, isn't there?

Q. I quote from Technical Paper No. 181, of the Bureau of Mines, on the Determination of Unsaturated Hydrocarbons in Gasoline, by Dean and Hill—which, I think, was offered as a whole—as follows:

"The gasoline to be tested is run into a clean, dry bottle, cooled for a minute or two by immersing in ice water and then 200 per cent by volume of ordinary concentrated sulphuric acid is poured in from a small gradute."

Now, you note, there, that the acid is not cooled, don't you?

A. Yes.

Q. And the gasoline is not kept cool—it is merely cool to start with. That is the fact, isn't it?

A. That is what it says.

Q. That is the standard test, isn't it?

A. What is the date of that?

Q. It is 1917?

A. Yes.

Q. That is the standard test, isn't it?

A. I think so, excepting that it has been modified slightly—along about five years afterwards.

The Master: That is what we have been talking about as the standard test?

The Witness: Except for the modifications in a later edition.

Q. And that is the standard test which you said, in your direct examinatoin, on page 9444 of the Typewritten Record, could be practiced on benzol, because the benzol will go into solution—you said:

"Now, it must be further borne in mind that using such a benzol, and following the ordinary care and method used in making sulphuric acid and petroleum products, we will not have a freezing of the benzol, contrary to the statement that has been made. I have tried it and it does not."

Now, you were talking, there, about the standard test, were you not?

A. I was, but I think it is—the test I have in mind is, I [fol. 1443] think, a later revision of that, and what might be spoken of as a more standard test.

Q. Doesn't that differ only in that the time of shaking is prescribed, and not the conditions.

A. It differs from that—I don't remember all the conditions about which it differs. That is an obsolete test, as you have it there.

Q. That is the test, and the kind of a test that you say you can practice on benzol—is that right?

A. Yes, on a benzol of the pure grade, such as we have.

Q. Now, in your prima facie case—and I go into this because you said that Mr. Wilson's counter-statement, that you could not do what you said, in your prima facie was untrue—in your prima facie testimony on page 1052 of the Printed Record, you said that when you ran this test you kept it in suspension in ice and water during the test for fifteen minutes. Now, can you do that and keep it liquid?

A. Yes, and it did.

Q. Keep it solid?

A. No, not keeping it solid.

Q. Keeping it liquid?

A. No—keeping it liquid, yes.

Q. Suspended in ice and water?

A. Yes, remembering here, using a pure grade of gasoline.

Q. Now, just quit—you did do it?

A. Yes.

Q. Now, you know that if you use pure benzol it freezes at the figure that you stated it did not freeze—isn't that true?

A. The pure benzol is a grade that boils at 80 and 82. It does not freeze at the figure stated there—42 F.

Q. I don't know what the meaning of pure is, in the benzol industry. If you take chemically pure benzol, when does it freeze—at what temperature?

A. We use——

Q. Now, just answer the question. At what temperature does chemically pure benzol freeze?

A. It freezes at 42 degrees—but not pure benzol. There is a difference between pure and chemically pure benzol, and I gave here, in my testimony, that it was the benzol boiling at 80 and 82, and I so stated.

Q. Did you state that in your prima facie testimony?

A. No, I think that it was where I gave the table and gave the details regarding it.

[fol. 1444] Q. Now, Mr. Neave asked you to make a test on benzol, and you came in and said you had made such a test on benzol. Now, you are trying to say that you did not make it in chemically pure benzol, but something else—is that correct?

A. I made it on the grade which is known as "pure," and which is the only——.

Q. And which is not pure?

A. And it is the only one available under ordinary circumstances, except as a chemical curiosity. You can get it——

Q. You have it in your laboratory.

A. You can get it if you ask for it—you can get it specially, from the chemical supply houses.

Q. Any any chemical supply house will send you essentially pure benzol that freezes at 42, will they not?

A. I doubt it. I imagine that you will have a great deal of trouble in getting it. Columbia University, for a good many years, had to import it, in order to get it. They couldn't get it in this country, and at present I think that there is but one firm that is making it.

Q. Now, in the tables, in Bacon and Hamor, which I cited, the sulphuric acid absorption was made with half the ratio

of the sulphuric acid that is commonly used in the standard test, wasn't it?

A. With half the ratio that is now commonly used.

Q. And, of course, that will give higher unsaturation figures, on the average, than the standard test?

A. No, it would do rather the reverse, because the—there would be a less quantity of sulphuric acid to act upon the gasoline.

Q. Don't you even know the elementary chemistry of this scheme, Doctor McKee? Don't you know that that statement you made is false, as a matter of chemistry?

A. That the use of less quantity will dissolve more material?

Q. Yes—why, certainly—don't you know that?

A. It is filled up here at the end—the chemical reaction is less; the final volume is the same; so that it would not dissolve the hydrocarbons.

Q. Now, don't yuo know—now, this is chemistry, this is not oil—this is just plain chemistry—don't you know, as a chemist, that if you use a lessor quantity of sulphuric acid you will, in general, on hydrocarbon oils, get a higher unsaturation figure?

A. I don't think that is true. You will remember, here, [fol. 1445] that while he adds that less quantity to start with, then the flask is afterwards filled with strong sulphuric acid.

Q. But not shake—just filled, to give you level?

A. And then the oil which finally remains there undissolved, is taken to represent the unsaturated.

Q. Sure. Now, let me just read—no, I want to call your attention to the language in this same standard, on the determination of unsaturated hydrocarbons, on page 11, of the Technical Paper, No. 181—speaking of the work of Egloff and Twomey, where they say that Egloff and Twomey recommend adding acid in small quantities to the gasoline, shaking and cooling thoroughly after each addition. I quote:

"In the experiments described herein, a considerable excess of acid was added rapidly to the gasoline. This procedure prevents inconvenient heating of the mixture and practically eliminates the troublesome factor of gas formation.

"It is, of course, natural to expect that if the addition of a little acid to gasoline produces heat and gas, the addition of more acid will produce more heat and gas. The preceding statements to the effect that keeping the acid in excess avoids such difficulties, may seem paradoxical, and the following suggestions are therefore offered in explanation of these statements. In the first place, the large heat capacity of the excess acid cuts down the temperature rise. In the second place it seems reasonable to suppose that the reaction should be considered as one in which the unsaturated hydrocarbons attack the acid rather than one in which the acid attacks the hydrocarbons. If the hydrocarbon concentration is low then the reaction is not violent and all that occurs is a combination, probably with the formation of sulphuric acid esters. If the hydrocarbons are in excess the reaction proceeds further and the acid is reduced with the formation of gaseous products, such as sulphuric dioxide."

Now, doesn't that indicate very plainly that if you add less acid you, in general, get a higher unsaturation number?

A. Let me see that, please.

Q. (Counsel hands witness book.) It calls for a yes or no answer. It can be answered yes or no.

A. That is not necessarily the conclusion there. It is that the acid is changed by the formation of gaseous products, such as sulphur dioxide. In other words, the acid is used up. This could be by the use of some hydrocarbons or the decomposition by the acid of the sulphuric esthers for by the excess of acid.

The Witness: It is either condition.

Q. Doctor, doesn't that state if you put in more acid you get less heat?

A. Yes, by carrying it——

Q. You have just told us if you have more heat you get a higher number in general.

A. Yes.

Mr. Wiles: If the court please, Mr. Dawson calls my attention to the fact that Paper 181 of the Bureau of Mines is Exhibit No. 418.

Q. Furthermore, in the tests for unsaturation, where you performed on the whole crude, if that crude contains considerable undistillable residues that give a higher re-action, a

re-action of the acid with those parts will raise the heat, and therefore, make the action on the distillable parts more violent, will it not?

A. Slightly.

Q. The things which you call asphalts in shale have actully been distilled in the process of manufacturing the [fol. 1446] shale oil, have they not—the whole shale oil is a distillate?

A. Yes.

Q. You cited extensively from Bulletin 210 of the Composition of Shale Oil. None of those shale oils are European shale oils, are they?

A. Shale oil made from Scottish shale they were in part.

Q. Were they in that book?

A. Yes.

Q. Made by the standard Scotch processes?

A. I think these ones that I cited of Scotch shale were made by laboratory scheme to parallel the products made by the standard Scotch process.

Q. Do you know about how they were made, or is that just a guess?

A. No, the method is described in the bulletin, and it is the Standard Bureau of Mines' method for producing shale oil from shale to give a product parallel to commercial products.

Q. And different commercial methods give wide varieties of unsaturation, do they not?

A. Of the methods used to make the commercial Scotch and French shale oil, the two happen to be the same, or to be substantially the same, using the Pumpherson retort, or a retort quite parallel to it or modified from it.

Q. Do you know of any American crude oil where a condenser temperature of 158 degrees Fahrenheit is required to keep paraffin in the paraffin distillates from freezing and clogging the condenser?

A. No, no American crude oil; but you can use somewhat lower temperatures.

Q. 100 degrees Fahrenheit is quite enough to take care of the paraffin distillate from American crude, is it not?

A. I am not a refiner, I cannot say on that.

Q. You should expect it to be, knowing the character of the paraffin distillates?

[fol. 1447] A. I doubt it. I do not know. I think you are a little low.

Q. And the waxy matter from shale oil has a higher melting point, frequently than the paraffin from American well oil?

A. I don't know what melting points can be obtained from American well oil, excepting in a rough way only.

Q. You do know that ordinary paraffin commercial wax melts at about 115, do you not?

A. I think for the coating of paper straws they are using an American wax at a higher melting point than that.

Q. Yes, the high melts go up to 135, as I remember, but the common mixes run 125 to 135, do they not—or 120 to 135, say, the common paraffin wax on the market?

A. If you say so, I have no way of disputing it.

Q. And you know that the waxes in the shale oil frequently run very much higher than that, do you not?

A. Not the ordinary wax from that.

Q. Some of them?

A. But they can make, just as we can make from the American, high melting waxes, say at 135, or whatever the figure is. And so from the shale oil they can make a high melting wax.

Q. There are some shale oil waxes that melt at higher than anything you get from American oil; you know that, do you not?

A. It is my impression that it is easier to make the high melting wax from shale oil than it is from American petroleum.

Q. Exactly.

A. But I do not know whether that is true as to anything excepting Pennsylvania petroleum.

Q. Don't you know, for instanve, that Catlin out in Colorado claims shale wax that goes away up to 160?

A. I never heard of it. I never recall hearing of it before, if it is so.

Q. But you do agree it is easier to make a high melt wax from shale than from petroleum?

A. I think that is true. I do not know positively of my own knowledge.

Q. But when the Barbet patent says they use 158 and have to use 158 to keep the condenser from clogging up,

wouldn't it indicate to you that perhaps they were not using American petroleum at all?

[fol. 1448] A. I could answer your question yes, if you would please off the last two words "at all".

Q. All right, I will leave off the last two words.

A. They might be using shale oil or crude petroleum, and that is evidenced elsewhere in the disclosure.

Q. I understand your position to be that when Pielsticker says when he wants to make very light oil or gas he uses the second or vapor phase coil, he does not mean by that that he is making gasoline; that is, your contention is that second coil of Pielsticker, the vapor phase coil, would not be what he would use if he wanted to make gasoline, but if he wanted to make something very much lighter, which is not gasoline?

A. I think his statement is a little different from that. Page 1671:

"If the manufacture of permanent gas or of very light oil is intended."

Q. Yes.

The Master: In the statement of his process, he was referring to the vapor coils.

Mr. Wiles: That is the one that speaks about it. That is the part.

Q. Your contention is that he does not mean there, when he says very light oil, gasoline?

A. Well, it would be extremely light gasoline for present day use.

Q. Not a modern gasoline?

A. No, it would be more like a gasoline of 1895 or thereabouts; a high test gasoline.

Q. And if he wanted to make modern gasoline he would make it in the regular still; that is your position, is it?

A. In the coil and retort combination, which I would call the regular still.

Q. Yes, in the liquid phase eventually. I mean the distillatory part of his apparatus and not the vapors?

A. I would think of this as a mixed phase there, just as many of our present-day operations are mixed phase.

Q. When you find Mr. Lang saying that he makes extremely light oil or liquified gas, you say that is gasoline, though. How do you reconcile your position on Pielsticker with your position on Lang?

A. I consider this as a gasoline, and I stated a moment ago it was gasoline essentially, or what we call a high test gasoline; a gasoline of the general type. I mean the type of 1895 that was a much more volatile gasoline than that of the present-day, though it is what we would speak of now as a high test, quite high test gasoline.

Q. That is the only reconciliation of those two statements?

A. Lang was nearly fifty years later than the other, and as such he would distinguish it from the gasoline in his day more than Pielsticker would from the gasoline of his day.

Q. Taking an oil which boils around, say, 720, 730 or 750, five pounds pressure is enough to double the cracking rate, isn't it, to permit you to double the cracking rate?

A. I cannot offhand give you the increase of boiling point of the oil by pressure. If you have got a table there, I would like to look at it.

Q. Of course, doctor, if you find that from looking at the [fol. 1449] exhibit my statement is correct, that five pounds rise in pressure—above atmospheric, I mean—will permit you to raise your temperature twenty degrees and double your cracking rate, then the use of a five pound pressure on the Pielsticker apparatus would have a very useful function, would it not?

A. Yes, it would double your rate, possibly.

Q. Yes, and without seriously increasing the danger of a hot bottom, even if you got one, from running crude oil?

A. Probably.

Q. You have told us that graphite is so porous that the oil would go through it if there were any pressure, in the Adams' apparatus, much like water through a blotting paper. Am I correct in stating your testimony? I only recollect it.

A. That is correct, if you would use pressure.

Q. And it would burn on the outside; you pointed that out.

A. Not unless it was set fire to.

Q. Well, it would go right out into the furnace, would it not?

A. Oh, if it was in a hot furnace, it would.

Q. And of course that would not do any harm?

A. It would mean that much loss of oil. If you had more than that it would—it would mean a good deal of harm.

Q. It would burn right in the furnace and add to the heat, would it not?

A. Oh, yes, so far as the furnace is concerned, that is right.

Q. It would not do any harm?

A. It would certainly——

Q. The loss would be simply of your charging stock, which is a relatively inexpensive product, is it not?

A. If that was the only thing, yes; if that was the only effect of it, the loss would not amount to very much money.

Q. You would simply use some of your cheap charging stock for fuel. Let me ask you another question about that. You also contend, I believe, that although this is so porous that the oil will just go out of it, leak badly, a little bit of gas forming the cracking will burst it. How do you reconcile those statements?

A. We know that in the electrolytic industry, where graphite is used for electrodes in the manufacture of chlorine and caustic soda, the major part of the loss of life of the graphite is due to gas bubbles, which are formed under the surface and which break off the graphite. This in the newer electrolytic plants, a few of them, has been over-[fol. 1450] taken to boiling its electrodes in linseed oil in come. I refer particularly to one in Niagara, which has order to fill up these pores, and another one in Naples, Italy, which uses naphthaline for a similar purpose. So there will not be gas bubbles formed underneath the surface and decompose the graphite. Thereby they have doubled the life of the graphite, and if hydrogen will do this, then certainly the hydrogen made from the cracking process would do it.

Q. At how high temperatures do those electrodes run?

A. Oh, they are running at temperatures below the boiling point of water. They are warm, but they are not hot.

The Master: That does not seem to me to answer the question. If the porosity of the graphite is such that the

liquid oil will go through it, the question is, why will not the gases go through it, and therefore obviate any likelihood of bursting the vessel?

The Witness: The reason, your Honor, is that graphite is one of those materials, which is much more readily wet with oil, or the surface of which is more readily wet with oil, much more so than it is with water. And the oil particle will clog up the outer pores through which the oil went in. Not permanently clogging them, but simply by capillarity the oil will hold it in. Just as we can have the holding of air in by a piece of cloth which is wet with water, as in the so-called water wings which the children use when they are beginning to learn to swim. We take the rather closed grained cotton cloth, we blow in air, and it does not come out because the surface of the cloth is made up of fibres which are wet with water, and the capillarity between the water is greater than the air will push out. You can by pushing hard on the water wings, so-called, push the air through the cloth. But you have there only a layer of cloth and water of perhaps one one hundredth of an inch, or one fiftieth of an inch. In the present case we have a layer of oil and graphite, which is, of course, much thicker and hence would give even more resistance.

The Master: That would be a matter of relativity between the tensile strength of your graphite and the amount of pressure you have inside of it, whether the gas comes through or not without bursting it.

The Witness: Yes.

Q. And you have tried this with graphite and you know it falls off, of your own knowledge.

A. I did not carry it out in just this way, but——

[fol. 1451] Q. That is all.

A. (continuing:) I ran a test, and the test showed it did do it.

Q. You never tried it under pressure in a graphite crucible?

A. No, it was not under pressure.

Q. Of course you described this electrolytic process—what was it for the manufacture of?

A. Caustic soda and chlorine. It was showing the matter of the porosity of the graphite and the flaking off of the graphite.

Q. And you told the Court it was hydrogen that did this, did you not? Did you tell the Court it was hydrogen?

A. I did.

Q. Just answer that.

A. Yes, but that was an error. It was gas.

Q. And you know it was not hydrogen?

A. Now that you call my attention to it, it was carbon dioxide gas. I thank you for correcting me.

Q. And carbon dioxide gas is a very much denser gas and with a very much slower rate of diffusion through porous membranes and substances?

A. Yes, than hydrogen; no, than the cracked vapors.

Q. In referring to the Gaster patent, you said it had pressure, as I understood you. Is that correct? Did you mean to tell the court that the Gaster patent disclosed pressure? Do you have to look that up?

A. My recollection is that the Gaster patent disclosed pressure in a minor way. I will look it up.

Q. Now, that is not what I got at. I have not had time to look this up, but did you intend to tell the court that Gaster disclosed the use of pressure?

The Master: Answer that yes or no.

A. Yes, on page ——.

Q. Just answer my question. Did you intend to tell the court that Gaster uses pressure in his apparatus?

A. I do not recall the wording used there, but my present understanding is that Gaster discloses that he can use pressure in his process.

[fol. 1452] Q. The fact is, is it not—and I am basing this largely on recollection myself—that Gaster has no disclosure of the use of pressure, but merely has a safety valve which will open if he does get pressure? Is that right? Correct me, if I am wrong, because I am stating this from memory.

A. No. In addition to the safety valve he states on page 1596, Petitioner's Exhibit No. 127, that he forces the mixture in by use of pressure, which he would not have to do if he was not using pressure in there. So there are two things, not one alone, that indicate it.

The Master: In the disclosure of the means for carrying out his process, is there any disclosure of the pressure?

The Witness: He does not show his pumps.

Q. And so far as using pressure to force oil into an apparatus, you know, do you not, that in an ordinary pipe still running at atmospheric pressure, you usually have to use, and do use about forty pounds of the inlet to get the stuff in?

The Master: You raise pressure to force the water through an open pipe.

Mr. Wiles: Certainly. I will withdraw the question.

The Witness: I beg your pardon. He does show pressure, your Honor, in the way of a pump here in the figure.

The Master: Do you mean pumping it in or holding the pressure in the system?

The Witness: This is apparently for pumping in, primarily.

The Master: That is the only ground for the inference that it is a special disclosure?

The Witness: And the use of the valves.

The Master: Safety valves.

Q. And the safety valve is put on every atmospheric still, isn't it, and set at one pound?

A. I do not know.

[fol. 1453] Mr. Cooper: That is in the evidence of the Government here. The Government's witness so testified.

Q. Dr. McKee, I have handed you a reprint of the Wilson-Balke paper exhibit, and have you now verified the statement which I made?

A. I think I will in just a minute.

Q. The statement, of course, with reference to the effect of five pounds pressure permitting you to raise the temperature twenty degrees?

A. That is right.

Q. You also said that Intschick disclosed pressure. Does Intschick disclose any more pressure than the few ounces commonly employed in the gas industry for pushing gas into a gasometer?

A. We cannot tell just how much he discloses. He does say on page 1549, "The gas entering under pressure, which it has in the retort," and then possibly elsewhere.

Q. And that is a gas making process, is it not?

A. Primarily so.

Q. And in the gas industries, the pressures commonly employed for running gas into gasometers are two ounces to the square inch?

A. May or may not. It is often many times that. It is sometimes much greater than that.

The Master: How much greater?

The Witness: Waukegan in Illinois, uses thirty pounds to the square inch.

Q. In a gasometer?

A. In their system, which, I think, is fed—I think they have a pressure gasometer, too. They must have.

Q. That is a high pressure gas distribution——

The Master: That is not a matter of inquiry. There is no use going into it.

Mr. Wiles: I do not think it means anything.

Q. You stated in the course of your testimony, on page 9375 of the typewritten transcript of the record, that the tar produced in a gasoline making and a kerosene making process is identical or the same, in substance. Perhaps I had better get it exactly. At the bottom of that page, your language was, "You get essentially the same tar in the same still working at the same pressure on the same raw material. What do you know personally of the characteristics of the tar obtained in a kerosene making [fol. 1454] cracking process, and a gasoline making cracking process? I want your personal knowledge on this subject.

A. I have had no personal experience in running either kind of apparatus, excepting in certain experimental work in connection with gasoline.

The Master: Laboratory work.

The Witness: Laboratory. Small plant work.

Q. You testified at considerable length in your testimony to the effect that cracking to make gasoline and cracking to make kerosene are essentially the same. Is that a correct resume of the general line you were driving at?

A. They are essentially the same process.

Q. You stated, I believe, that aside from re-cracking—at least I got this impression and I want to see if I understood you correctly—aside from recracking of some products, if you start with the same stock and crack at the same temperature, you are bound as the initial product to get gasoline and kerosene in certain fixed proportions.

A. That would not hold if we cracked kerosene to get gasoline, but if we take, say, a gas oil, and the initial product before being fractioned out, or run through a fractionating tower, from the top of the still, or anything of that kind, we would get by heating our gas oil to the same temperature under the same conditions, essentially the same proportion of products, yes.

Q. And you would get also the same proportion of gas, of course, would you not, by the same reasoning?

A. Essentially so. Of course it would depend somewhat upon the type of process used.

Q. Well, the same general type of process.

A. Yes.

Q. I think that is generally correct, Dr. McKee; I do not see how it can help being. So I am not quarreling with that finding at all. I am merely wanting to use that as a starting point. The initial product of your re-action in the gas oil must be a fixed quantity of gas and gasoline and kerosene, unless you re-crack some. We start with that as an agreed proposition; is that right?

A. I think so.

Q. Of course, it follows that any re-cracking of anything will give you more light products and less kerosene and more gas?

A. More gas, and add something to your heavy products.

Q. Yes, but any increase in the ratio indicates re-crack-
[fol. 1455] ing in that original—what you might call the natural ratio of the primary re-action?

A. Yes, or carrying your primary re-action further.

Q. Yes, but if you merely carried the primary re-action further you will simply get more in the same relative proportions?

A. Yes, that would be true.

Q. So if we have X per cent of gas and Y per cent of gasoline and Z per cent of kerosene, if we just crack fur-

ther without re-cracking anything, we will keep X, Y and Z in the same ratio to each other?

A. That would be true, if we include as our re-cracking stock, both the heavy and the kerosene.

Q. Therefore, this natural ratio—and I like to think of that in my own mind, and have for years thought of that in my own mind as the natural ratio—you understand what I mean, then, that they would be the primary immediate products. They will occur in the natural ratio, with the kerosene in the highest proportion to the gasoline, and as you re-crack the kerosene portion will go down and the gasoline portion will go up, necessarily.

A. That is the direction in which it takes place.

Q. So that if we want to get light on what that natural ratio is, of course it must be the very highest kerosene ratio that we can find in a cracking process of the type under consideration; that is correct, isn't it?

A. It will approximate that.

Q. Yes.

A. Of course, even that process may do some re-cracking.

Q. Yes. In other words, it is hard to get a kerosene ratio as high as this natural ratio, because some may re-crack.

A. Yes.

Q. When you are running for kerosene, maximum kerosene, of course what you will naturally do is to get your kerosene out of the still just as fast as it is formed, so as to avoid this re-cracking in the making of what would then be a relatively useless by-product, gasoline; that is correct, isn't it?

A. That is what ought to be done.

Q. Surely, and you would naturally try to do it?

A. I think one should do it, yes.

Q. And you would produce, therefore, kerosene and gas- [fol. 1456] oline in as near the natural ratio as you could possibly produce them?

A. Yes. You might not, of course, pay any attention to making complete condensation, or saving of your gasoline. You are after your kerosene.

Q. Now, what is the natural ratio?

A. I cannot give you that in figures.

Q. In an ordinary coking still you are bound to get your kerosene in somewhere near the natural ratio, or even less—somewhere near the natural ratio. That is correct?

A. Well, I note that in Bell's American Petroleum Refining he seems to indicate that the amount of gasoline and kerosene obtained in a coking still is insignificant in amount. In other words, that there is not any appreciable cracking or not appreciable cracking. The amount of cracking is quite small, so small that he puts it in with the gas oil.

Q. Don't you know that the natural ratio approximates four or five times as much kerosene as gasoline?

A. No, I do not.

Q. If it be true that the natural ratio produces kerosene to four or five times the extent of the gasoline, then the problem in reaching our present-day processes is to decrease that ratio by about ten, isn't it, eight to ten; that is, to go from five to one kerosene, to 2 to 1 gasoline?

A. I do not know what the plant yields of kerosene are in the cracking for gasoline processes.

Q. You do not know either ratio, either, the present day——

A. As I said, I do not know the numbers in either case.

Q. If you have to make this increase, such as I have suggested, and you say you do not know, but which is shown of record, you will not only have to recrack your kerosene to a very large extent, but you will have to crack a much larger quantity of the original charging stock, will you not?

A. Than what? Larger than what?

Q. Larger than if you are making kerosene only.

A. I would like to have the question read.

Q. Oh, I will strike it out. I guess it is not clear.

Q. If you have to increase the gasoline ratio, to the extent I have indicated, then you will have to crack the original stock simultaneously, to a very much greater extent, as well as merely recracking the kerosene—isn't that true?

A. Yes, because to crack the kerosene you simultaneously——

[fol. 1457] Q. Well, I don't care for your reason. I just want the facts. And while you are cracking, at all times you are forming, beginning with your primary reaction, a certain amount of asphaltic material, are you not?

A. Yes, a small amount.

Q. And that is dirty coke forming material?

A. Yes.

Q. Now, I want to call your attention to "Exhibit No. 417."

The Master: Before you begin on that, I would like to get hold of the twine oil patent.

Q. (continued). In your direct testimony you again asserted that in your judgment Aisinman discloses oil having both high and low viscosities, with a single gravity, and vice versa—am I correct in that?

A. Yes.

Q. Now, if Aisinman intended to disclose anything of that kind, it would be an oil from the single distillation of a single product, differing from anything known in the oil industry theretofore, would it not?

A. Not necessarily.

Q. Wouldn't it be so extraordinary that you would have, normally, to use some special method of calling attention to the eccentricity of the oil?

A. No. That is a useless type of oil, or nearly so, and he would pay not particular attention—excepting that he did make some measurements on it.

Q. It is directly in the ordinary lubricating oil range, is it not?

A. The ordinary lubricating oils have much higher viscosities for the same densities.

Q. Now, if you interpret Aisinman as Mr. Wilson interpreted Aisinman, in each case associating the light oil with the thin oil, then the Aisinman series falls right in with ordinary, common American oils, does it not—as shown on the chart—the Aisinman oils being the solid triangles and solid dots, being the ordinary American oils. Now, we see that Aisinman falls, on the chart, aside from the beginning of his series, directly among the American lubricating oils—do we not?

A. If we use that interpretation, yes, but if——

Q. Now, that is all. If you use the interpretation Mr. Wilson gave—both he and Dr. Lewis said it was the natural interpretation—then the oils are just the ordinary [fol. 1458] oils falling right within the range of ordinary lubricants, aren't they?

A. They fall within the range of the products mentioned by Burton in his patent—but not all American lubricants.

Q. They are perfectly typical of similar American lubricants, are they not?

A. I don't understand your word "similar," there.

Q. Well, their gravity and viscosity puts them squarely among common, ordinary, everyday American lubricants, don't they? If you interpret Aisinman as Wilson and Lewis interpret it.

A. For these principal ones, yes.

Q. And the other one is a kerosene and goes right along with the ordinary kerosenes, does it not?

A. In that interpretation, yes.

Q. Now, you read Aisinman not only to disclose those common, ordinary oils, but to disclose two freaks, or four freaks, such as you cannot find described anywhere else in the world, don't you?

A. Because Aisinman say- they are aromatics, and lubricants are not aromatics.

Q. Well, you didn't answer my question, at all. If you interpret the Aisinman disclosure as you interpret it, you derive from it, four oils of characteristics which you cannot find described in any other publication in the world—isn't that true?

A. No. I found them, but you would not permit me to bring them out in my direct, on that. I found them in the Bulletin 210 of the Bureau of Mines.

Q. Recent ones? Nice new oils?

A. Scotch shale oils.

Q. Nice new oils?

A. No, they are the regular old Scotch shale oils.

Q. Can you find that Aisinman, if interpreted as you interpreted it, discloses four oils of characteristics such as you cannot find in any publication prior to Burton?

A. I haven't found them in any publication prior to Burton. But the oils existed, nevertheless.

The Master: You mean that it is potentially existent?

The Witness: No, I know that those oils exist, excepting that they don't happen to be described in Burton's special terms.

Q. Their properties were not given, at all—you can't find any viscosities, or gravities, at all?

[fol. 1459] A. The viscosities, according to the Saybolt instrument were unknown excepting to the Standard Oil group. This was a secret instrument that no one else could use at that time. This instrument was not available to the public.

Q. Every one of those are translated from Engler to Saybolt, are they not, by your own figures, and you say that those are the best in the world.

A. No. These are the figures given by Burton—most of those are.

Q. The prior art is all translated, isn't it, by your own figures, that you produced on direct?

A. Yes.

Q. Now, if your interpretation be right, then you have to insist, with equal certainty—with equal positiveness, on what we have called imaginary oils, of high gravity and low viscosity—that is what Mr. Wilson called the imaginary giants, as well as the imaginary dwarfs.

A. Yes, because Aisinman speaks of them as aromatics, and aromatics would not be in this field, at all—they would not be up here in the lubricating field. They would have higher density.

Q. The other end of your series, when you switch these ends—let us just take these two—126,702 and 168,667 on the chart—which we say are the real oils—now, when you switch the ends of the series you get these other two, 37-412 and 404,459, and you have got to say that they are both disclosed by Aisinman, don't you—and you did so say in your direct examination—because those figures are taken right off of your prima facie testimony?

A. I think that that is correct.

Q. Now, this lower one, down here, you say could be obtained by the presence of aromatics—that is, you would get the heavy oil without getting the thickness—is that correct? That is, that is one that you say aromatics would produce?

A. An oil having aromatics in it would be in that direction.

Q. Yes, and that is the one that you say Aisinman discloses?

A. That is one of those.

Q. Now, if Aisinman discloses this one, 37,412, he also discloses 404,459, doesn't he?

A. I presume so.

Q. Now, don't you know it to be a fact that there never has yet been an oil—and you cannot find one in the world, [fol. 1460] that answers to this imaginary oil of yours, up here, 404,459, which has a gravity of 45, of kerosene, and a viscosity of 130, of lubricating oil—don't you know that the consequence of your disclosure is to postulate an oil such as the mind of man never dreamed of before?

A. I don't think so.

Q. Now, have you the slightest scintilla of evidence that anywhere in history there was an oil, of kerosene gravity, with lubricating oil thickness, which you say Aisinman tells about? And how can you make it?

A. I doubt if anyone would be interested in such a type of oil, and would take the trouble to measure it, if it could be obtained. I think it is in the same fix as these oils were in times past—no one was interested in measuring these high gravity, low viscosity oils, and as such we have almost no disclosures of record, though the oils were, of course, well-known in the previous times.

Q. Now, if you were given all the oils in the world, today, where *you* could you find an oil with a 45 Baume kerosene gravity, and a lubricating oil viscosity of about a hundred and thirty, that you say you find in Aisinman?

A. I am not able to pick out oils, here and there—I mean, I haven't looked for them. You say I can't find it, but I don't say so.

Q. You don't know of one?

A. I cannot, offhand, give you one, any more than I can, offhand, give you one of these, here, directly—of these other oils, which you say are quite common.

Q. Now, when you recur to the Aisinman article, you find that the aromatics came over in the very heavy ends, don't you?

The Master: I think this is all largely academic, in view of the patent, itself.

The Witness: At page 1488, he speaks of that as in the heavy ends. On page 1486 it is not. He speaks of them—"Under these conditions rearrangements and formations of aromatic hydrocarbons are readily produced at the same time."

On page 1488, it speaks, in the upper part of the page, regarding the paraffin products, and in the middle of the page, about the aromatics, near the end of the run.

The Master: It would seem to me that this discussion is academic, unless you contend that Aisinman discloses this process: "Distilling bil having a boiling point of upward [fol. 1461] of 500 degrees F., at a temperature of about 650 degrees, to 850 degrees F., and under a pressure of upwards of three atmospheres, until about 65 per cent of the oil has been distilled over." Does Aisinman disclose that as an initial step?

The Witness: No, not in detail, at least.

The Master (reading): "Then drawing off the residue from the pressure still, distilling the latter under approximately atmospheric pressure and condensing and collecting the distillate." Does he disclose that?

The Witness: He discloses that.

The Master: He discloses the second, but not the first step?

The Witness: He does not disclose the temperatures and proportions.

The Master: If he does not disclose them, then the disclosure of the second step would be of no avail. Of course the art is perfectly at liberty to use Aisinman, and if it can produce the article of the first claim by the Aisinman process, without infringing upon the process of the second claim, they are at perfect liberty to do so.

The Witness: I think that is really what is happening in here, rather than the process. He doesn't give the details of the process.

The Master: So that the whole discussion is academic.

The Witness: The whole discussion is on the product.

Mr. Wiles: It goes to the product claim.

The Master: All three claims are on the product. It is not only a new article to manufacture, but it is a new article of manufacture produced as the result of that process.

Mr. Wiles: Certainly, in the narrower sense.

The Master: So that, leaving out of view, merely the first claim of the patent, unless Aisinman discloses the process of that article produced by the process, he does not meet the patent in suit, does he?

The Witness: He does not disclose the first half of the process in detail.

The Master: Now, of course, going further than that on Aisinman—it is not what Aisinman might, or would produce. There should be some evidence that he actually did produce the formula of the first claim. Is there any evidence on that?

The Witness: Yes, sir. He describes a product which fits the formula of the first claim, as well as the oils that Burton describes.

Mr. Wiles: If you switch the ends of the series.

The Master: You can't do anything of that kind. You have got to take the product as Aisinman describes it, and as Burton describes it, and put the two side by side, and not what might, or could have been done, if something else had been done.

Q. Now, Doctor McKee, in discussing Pielsticker, you said, I believe, that you could identify the volumn *w*, as a bubble tower, because of the exterior appearance of it. That is correct, is it?

A. That its exterior agreed with the ordinary type of bubble tower, rather than with any other type of tower.

Q. But it does agree with both, doesn't it?

A. No.

Q. It does agree with perforated plate towers, and with baffle plate towers, such as were, and now are in use, in exterior appearance, does it not?

A. In the case of bubble plate towers and perforated towers, the units are normally closer—the sections are only a short distance apart. In the case of baffle towers, they are ordinarily relatively longer.

Q. But sometimes they are shorter, and sometimes they are longer, aren't they?

A. In the case of bubble plate towers you have the sections short. The plates are put in, and the space for bolting them is fairly wide. In the case of baffle plate towers you ordinarily do not use that construction, but you may take it up of sections in which case the riveted, or bolted portion is quite narrow.

Q. And sometimes the sections are just as short in one as the other?

A. I don't recall of ever having seen a baffle plate tower, [fol. 1462] with sections even of twice the length of the other—that is, they are ordinarily very decidedly longer.

Q. And based on just that exterior appearance, alone, you are able to say that Pielsticker positively and certainly discloses the bubble tower?

A. That would be a normal explanation of it, but particularly when he adds to it "like used in the benzol industry."

Q. And you think that is absolute, positive and certain and could not be anything else?

A. I think it is 99 chances to one.

Q. That is not enough to anticipate a patent?

A. Well, or more.

Q. Yes, I think you might assume that, Dr. McKee—are you certain——

A. I am, in my own mind, that Pielsticker's is a bubble plate tower.

Q. Now, calling your attention to the title page, I will ask you to look at the drawing on page 642 of "Die Industrie des Steinkohlentheers"—I will ask you to look at the picture, first. Now, what kind of a tower is that? That is in the benzol industry, I might tell you—what is that, from the external appearance?

A. From the external apearance I would say that I could not tell, but it probably is not a bubble tower, judging from the character of the exterior. I cannot say for certain, but it is probably not.

Q. Now, how does that differ, essentially, from Pielsticker, in external appearance?

The Master: Isn't this a good deal of guessing about draftmen's symbols in representing structures?

Mr. Wiles: I think so.

The Master: Isn't that out of the ordinary, in a patent case?

Mr. Rauber: We don't depend just on that one thing for our position that this is a bubble tower.

The Master: You have something in the text that you rely upon for the inference that it is a bubble tower?

Mr. Rauber: Yes.

The Master: Then, why spend a lot of time about comparing smymbols here?

Mr. Rauber: We don't think it is necessary.

Mr. Wiles: The only thing that it is in the text that it is the benzol industry.

[fol. 1463] A. That isn't the only thing. Answering the question. The vertical distance of the units is very much higher, proportionately. The exterior portion, on the width of these flanges that stick out, is much less, proportionately. In this it is customary to have those wide enough so that it can be opened up and closed easily. And here, where you have it relatively high, as this is, and large in diameter, if you increase your diameter as you have here, you would not increase your height in the fractionating column, but you would in the case of a bubble tower, or other types of towers. In other words, the height of it is independent of the width.

The Master: There is nothing on this drawing that tells anything about the height or width.

The Witness: Excepting the pipes and valves.

The Master: You have, I think, time and again, pointed out that these diagrams of patents, are not like working drawings, and that their dimensions can be only guessed at.

Q. I want to call your attention to page 644 of the same work—and the German matter on it—which I assume you can read—and I will ask you if it is not a fact that that tower is not a bubble tower, but is a perforated plate tower, and if it is not in the benzol industry.

A. Fig. 181—I see no perforations are indicated, but it is presumed that they are. I don't find a description of 181 here.

Q. You will find it on the other page, probably.

The Master: Whatever the Doctor may say about it, counsel for the petitioner will understand that the burden is on the petitioner to show clearly the structure of the patent in suit, in the prior art—not by guessing, but by actual description. You cannot import into a structure, anything, by hypothesis, or by guess.

Q. It is a perforated plate tower, in the matter that I have shown you, is it not?

A. 181 is.
Q. Yes.

Mr. Wiles: That is offered as "Exhibit No. 612."

The said pamphlet, so offered, was admitted and read in evidence, was marked "Exhibit No. 612," and the same is herewith returned.

Mr. Wiles: Now, we offer in evidence "Letters patent No. 1,366,956," of February 1, 1921, being the patent issued to Joseph Schneible, of Chicago.

[fol. 1464] Mr. Rauber: I object to that as being more recent than the patent in suit, and it could not prove anything as to what went on in 1917, or earlier.

The Master: The inference that Dr. McKee drew from the diagram drawing, was unwarranted, in view of the other evidence. It goes to draftsmanship.

It shows that the inferences that Dr. McKee makes from the drawing, as having been prior, are unwarranted inferences. The objection is overruled.

The said patent, so offered, was admitted and read in evidence, was marked "Exhibit No. 613," and the same is herewith returned.

Mr. Wiles: I wish to offer in evidence pages 130, 79, 81, 82, 84, 101, 113, 106, and 107 of a book entitled "Shale oil," by Dr. McKee.

I am perfectly willing to submit these to Dr. McKee for any explanation that he may care to make. I have ordered photostatic copies of these pages, but they have not yet come.

The said pages, so offered, were admitted and read in evidence, were marked "Exhibit No. 614," and the same are herewith returned.

The Witness: I wish to call attention to the fact that the last four pages offered are from chapters of the book, which I didn't write. The first four pages offered are from a chapter of the book which was, in large part, written by Mr. Goodwin, of the Standard Oil Company of New Jersey, and in which I wrote a part.

The Master: You adopted it all, though?

The Witness: No, I disagreed with a good deal of it. I don't know what is said on these different pages, but I have made no pretense to agreeing with all that is in the chapters written by the various men, who didn't agree among themselves, and it has been so stated before.

Mr. Rauber: Is that in the preface?

The Witness: Not in that many words, but——

Mr. Rauber: It is understood?

The Witness: It is understood, and that is quite common in these American chemical societies monographs, such as this is.

The Master: Just a moment. Where those articles by other authors appear, is there, in that text, any dissent, that is not included in what is offered—any criticisms, or dissent?

The Witness: No, your Honor, excepting that the chapters, themselves, dissent from one another, and the book is not by myself. It is, as the title shows, by Ralph H. McKee, R. T. Goodwin, W. A. Hamor, and others.

Mr. Wiles: You were the editor of the book?

The Witness: Yes, sir, I was the compiler of it.

Mr. Wiles: And you were the joint author of one of the chapters.

The Witness: Of one of the chapters, yes. I was the joint author of the chapters containing pages 79, 81, 82, 84, and had practically nothing—I had nothing to do with the writing of chapters containing pages 101, 106, 107, 113, and 130.

[fol. 1465] Redirect examination.

Questions by Mr. Rauber:

Q. Have you had an opportunity to examine the second and third impressions of that book, by Robinson—I mean, to compare the second and third prints?

A. I have.

Q. They are substantially the same, are they?

A. They are—in fact, so far as the portions that we were discussing, I think they are exactly the same.

Q. Is there any inconsistency between the paragraph on page 52, in Robinson's book:

"The base column should have its reflux entirely in the condenser above the column, and the entire reflux returning through the entire length of the column."

On the one hand, and the figures on pages 147 and elsewhere, and your statement that the reflux should return below the top plate.

A. I see no inconsistencies in that.

Q. What determines where the reflux is to be returned?

A. In a column still we are distilling off the lighter products—say, gasoline in one case and alcohol in another case. This passes up to the reflux—the vapors pass up to the reflux condenser, which condenses part of the vapors—the heavier part—allowing the lighter and purer part to go on to the final condenser.

If the purer parts go on to the final condenser—meaning pure alcohol, or lighter gasoline—then we are getting a less pure part reflux—that is, an alcohol carrying more water, and a gasoline carrying more kerosene: It should then be returned to the plate which carries the same composition of impure material, as the reflux. If it was returned to the top plate it would be returned to a plate which is giving off vapors purer than it is, and here we would want to return it to the plate—which may be the second or the third, or even the fourth plate, though, ordinarily, the second, or third plate of the column, in order that we may not render the top of the column less pure, by putting in the dirtier product—dirtier in the sense of carrying more kerosene—in the case of gasoline—or, more water, in the case of alcohol.

[fol. 1466] From there the reflux should flow down the entire length of the column, as is stated in the first part of the book, rather than as was sometimes earlier done, put in at some other place.

Q. Referring to the tests made on the material from the Cross cracking coil, will you state the circumstances under which you obtained the material that you tested?

A. I obtained the material from the commercial plant, by directing a man to take samples in a certain way, and from certain parts of the apparatus. I obtained the samples from the demonstration plant at Kansas City, by assisting in taking the samples, myself.

Q. On page 9581, of the Typewritten Transcript, in speaking of the conditions in the heating coil of the Cross process, you were asked, "Did you see those conditions?" Is it possible to see the condition of material in a Cross coil?

A. No.

Q. Why isn't it?

A. The pressures and temperatures are so high that we cannot readily use—or, we cannot, so far as I know, use transparent containers to carry out the process. Moreover there are other conditions, such as the separation of the film of carbon and the dark color of the products, which would prevent one seeing readily what was taking place, even if we could use, at these high temperatures, approaching red heat the——

The Master: It is not a question of what you could do, but it is a question of what you did do.

Mr. Rauber: I asked him if he could see it.

The Master: And he said he could not, and that would depend on the demonstration plant at Kansas City—whether he could see it, or not, would depend upon the construction of the demonstration plant at Kansas City. If it was a closed pipe of course he couldn't see it.

Mr. Rauber: I asked him if it could be arranged to be seen, and he is giving his reasons why it could not.

Q. Can the condition of the material in the coil be seen by drawing off a sample?

A. No. You have changed your conditions when you draw them off.

Q. How can the condition of the material in the coil be determined?

A. By taking certain measurements of temperatures and pressures within the coil, by taking out a sample of the [fol. 1467] product and determining the properties of that, and then from those properties and knowing the temperature and pressure, you can calculate what is present there, within the coil.

The Master: What took place in the coil, then, is simply a matter of inference, or deduction?

The Witness: Deductions from known facts, and they can be mathematically calculated with exactness.

Q. Can those conditions be determined by the condition of the material in the coil and the reacting drum into which it discharges?

A. No.

Q. Can the conditions in the reacting drum, themselves, be seen?

A. No, for the same reason that you cannot see them in the coil.

Q. If liquid were present in the reacting drum, would that necessarily prove that the condition of the material in the coil is liquid?

A. No, because the temperature could be lower, and, in fact, is lower.

Q. In determining your critical temperatures in a glass tube, would the bursting of the tube give you an indication of the pressures used?

A. Yes.

Q. Will you describe the changes observable in cooling a tube, that indicates that the critical pressure has been reached within the tube, and compare them witht he phenomena when vapors below their critical pressures are cooled?

A. The cooling which we carried on here in our carbon dioxide tube is one which will illustrate the phenomena. When we have the product under the critical conditions—that is, at the critical pressure or above—and start to cool, we have the vapor changing to a fog through the whole mass and then coming to the liquid condition with the manniscus at the original starting place, whereas, if we cooled below the critical pressure, that is where we have the vapors, we may say, under ordinary conditions and start to cool, we will have the liquid condensing on the side of the tube, running down, and the manniscus rising up from the bottom to the top, until we have reached the original condition. The two phenomena are quite different. There is no great confusion of the two.

Q. On page 9887 you gave the solubility of carbon dioxide in water at ordinary temperatures and pressure as about [fol. 1468] three or four per cent by weight. Have you checked up on those figures?

A. Yes, and I find that they are in error. They were given from memory. The solubility is only about one-sixth of one per cent, at a temperature such as there is in this room.

Q. Does the cracking of a gas oil reduce the critical temperature of the gas oil?

A. It does.

Q. Under all circumstances?

A. Unless you would simultaneously distill off the more volatile portion.

Q. If the gas oil were cracked in a pressure still of the Burton type, would the critical temperature of the oil be reduced?

A. The critical temperature of the oil in the still after the cracking had taken place for some time and gasoline distilled off would be higher than it originally was.

Q. Why is it higher rather than lower?

A. Because you have formed heavier products as well as light products from the gas oil, and the average boiling point of the residuum material is higher than the starting material.

Q. What is the essential feature, then, in reducing the critical temperature by cracking?

A. The production of a lighter product in larger amounts than you produce heavier products. That is, there is more effect from the gasoline and gas produced than there is from the heavier tar produced simultaneously.

Q. On page 9706 of the typewritten transcript of record, Mr. Wiles asked you the question:

"Q. Look at the drawing I now hand you and state whether or not that is the drawing of the apparatus that you referred to as a Cross apparatus in the testimony yesterday afternoon (handing witness paper)?

"A. It is, and I should add that it is——

"Q. You do not need to add anything. That is all I have asked you."

What was it that you wanted to add at that time?

A. That this was the drawing of the demonstration plant and not of the commercial plant, from which the samples were taken.

The Master: I thought you said you took the samples from the demonstration plant?

The Witness: I did as well. But the previous testimony, [fol. 1469] I think, had not specified which of these two plants it was. I actually did take them or have them from both plants.

The Master: Did you take them yourself from the commercial plant?

The Witness: No, I directed a man to take them for me.

Q. On page 9707 you were asked this question:

"Q. On page 9254 of the typewritten record you were asked about the relative efficiency as fractionating devices of the Humphrey's run-back and still-head of the Dewar & Redwood still. In your answer beginning on page 9255 of the typewritten transcript of the record, you quoted a paragraph from Young's 'Distillation Principles and Processes', beginning on page 127. Then you skipped and quoted another paragraph, which is at page 9257 of the typewritten transcript of record.

"Do you consider that you by those quotations fairly and honestly reflected Young's views with reference to reflux device of the character shown in Humphrey's patent?

"A. Yes, so far as compared with Dewar & Redwood. Young does, however, state——"

And then you were interrupted. What explanation do you wish to add?

A. Young does, however, state that a sloping one, if the ends are bent, is more effective than a straight one. But he calls attention immediately following that it should be to give still greater effectiveness, made like a spiral; and, further along, for still greater effectiveness, it should be a bulb type, and he illustrates a number of bulb type still-heads.

Q. Of what were the advantages stated for the spiral and the other types of stillheads?

A. Because, unlike the straight pipe, either sloping or vertical, they mixed up the vapors. That is, they produced eddy currents and gave a more uniform temperature through the pipe, so that there would not be a core of unchanging gas passing up through the center of such a pipe as in a straight pipe.

Q. Would the stillhead of the Dewar & Redwood apparatus be such as to provide eddy currents also?

A. It would do that quite effectively.

Q. On page 9711, you were asked the question:

"Q. You did not quote the succeeding paragraph which commences one line beyond where you ended, that is, entitled 'sloping still head', and distinctly referred to the kind of general type of fractionation on the Humphrey's [fol. 1470] device, did you?

"A.I did not quote it, it was, however—may I explain further?"

Do you wish to offer any explanation other than you have just offered on that?

A. The explanation I have just offered covers it.

Q. Have you any reasons for believing that the Dewar & Redwood stillheads of exhibits Nos. 103 and 107, is of a bulb type, rather than a horizontal pipe?

Mr. Wiles: That is purely speculative.

A. It would be the natural inference that it was to anyone who was experienced in this field; and this is confirmed by the drawings themselves.

Q. We do not need to go into it in much detail, doctor.

A. Well, such a stillhead could be readily cast with a sweep casting, which is the cheapest type of casting, you can make the casting without making a pattern; and as such it is an inexpensive type. Moreover there are bosses at the top of this part of the drawing, where the safety valves and pressure gauges and outlet pipes are, just as would be made for a casting and not as would be made for a pipe. Again, it would be extremely difficult in the case of a pipe to round over the ends and close the ends there, without having an opportunity to put a mandril in on the back side when you are hammering on the outside of it. In the case of the retort itself, that is not true, because it has an open end and the mandril could be put in from the outside. At the present day it would not be so difficult, but at the date of 1890 it would be exceedingly difficult and today we have acetylene and electrical welding that would permit us to carry out this operation in other ways, but they did not then use those for such purposes.

Q. Doctor, relative to the recovery of spend acids, what is the purpose of such recovery?

A. To permit the re-use of the acids in the process.

Q. Are there other ways of purifying it for this purpose, than that of the Commercial Construction Company, which you classed as one of the modern methods?

A. Yes, there are other methods as well.

Q. Dr. McKee, do you know anything relative to the costs of recovering——

The Master: By a refining company.

Q. (continued). The costs of recovering acids by these different methods?

The Witness: I think that I can say that while I have not had to do with the operation of such plants in refineries, I have had to do with some operation in sulphuric plants of nearly the same type, and know the relative costs of modern concentration as compared with the old. For example, a company with which I have been connected for ten years has several concentration plants, and in fact in that time has had probably eight or ten concentration plants of three or four kinds.

Q. With reference to the Lang patent, does condensing under pressure increase the light ends of the light distillate?

A. It does.

[fol. 1471] Q. Do the light ends increase the volatility of this light distillate?

A. They do.

Q. What does Lang say as to the function and purpose of his apparatus as a whole?

A. To produce a very volatile naphtha or—I do not know whether he uses the word "naphtha", but a very volatile product.

Q. When you said on page 9374, "Yes, it increases the light ends of the gasoline and makes it very volatile, just as Lang says", on what did you base your statement?

A. On essentially the whole patent and the process it describes.

Q. On page 9743, Mr. Wiles asked you this question:

"Q. Do you mean to tell the court as a fact that the modern cracking plant—say the Holmes-Manley—cannot take a gas oil stock and make out of it only gasoline, and tar, of course?

"A. It makes——

"Q. Now, listen—did you mean to say that to the Court?

"A. Yes, though I want to add an explanation."

What explanation do you wish to add?

A. That such a plant will necessarily make kerosene as one of the products, just as we discussed in the examina-

tion this morning, but that it may use that kerosene in any of a number of ways. It may use it for recycling and cracking, or it may refine it and put it on the market. It must, however, when making gasoline, actually in the cracking process make kerosene.

Q. Are the heats observable to supply the latent heat of vaporization and the heat of cracking the only factors upon which you base your opinion that the heating in the coil [fol. 1472] and in the tank of Pielsticker, are not equal—that is, the rise in temperature, are not equal?

A. No, you have in addition that the heat transferred between the gases in the furnace and the oil in the retort would be much less, for several reasons, such as the fact that the oil is at a higher temperature, consequently the differential of heat between the furnace gases and the oil would be less; second, that the shape of the vessel is such that per unit of volume of oil the vessel is not fitted for an efficient heat transfer as the coil, and possibly other reasons.

Q. Would it be possible to pass an oil or other liquid through a heated coil and heat it so that its outlet temperature might be 300 or 400 degrees Fahrenheit, while at the same time it might be locally heated in coil to a much higher temperature?

A. Yes, the outside layer of oil when passing through a heated coil might easily be heated very much hotter than the center of the oil in the coil; that is, the core of the oil in the coil. The outlet temperature might be simply the average temperature.

Q. Does the fact that you have found asphalts in shale oils necessarily mean that asphalts are distillable?

A. No. The shale oil has been distilled from the shale, but the asphalt may have been and in fact was formed from the shale oil when it is redistilled or during the redistillation the second time, or even during the condensation process. Just as in the cracking of oil, we get asphalt even though the oil itself before cracking had been distilled.

Q. Do you still have in your possession, Doctor, the physical exhibit No. 251, of a shale oil?

A. I do. A shale oil gasoline.

Q. A shale oil gasoline?

A. I do.

Q. Do you find that it has changed color to any appreciable extent?

A. So far as my memory serves it is approximately the same color,—or is of the same color as it was when presented to the Court a little more than a year ago.

Q. Do you find any evidences of gum in it?

A. Yes, there has been some separation of a small amount of material. This small amount is almost, though not quite, colorless. It has a very slig[illegible] yellow tint to it, this stuff at the bottom.

The Master: It is the same sample that we applied to the Standard?

Mr. Wiles: Oh, no. You see, he made one that was so off color that it would not pass any test at all which he said was typical, and then seeing that he was in a hole, he brought in another sample that he specially over-refined, and now that has gone and gummed up everything.

Mr. Rauber: I object to the attorney testifying in the case.

Mr. Wiles: I am not testifying.

The Master: That is part of the argument that he will make some day or other.

Mr. Wiles: There is no question about what happened. The first sample he said was typical, but it would not do at all. Is that all?

Mr. Rauber: That is all.

Q. You had that in your possession, that sample, all along, had you?

A. Yes, it has been standing on a shelf in my laboratory, exposed to the light, since the Court adjourned a year ago, a little over a year ago.

[fol. 1473] Recross-examination.

Questions by Mr. Wiles:

Q. You now say that the asphalt which turns up in shale oil on the attempt to distill it is formed in the redistillation; is that right?

A. Or, I said, while it is hot in the condensation.

Q. It is a subsequently polymerized product?

A. I do not consider that it was in that form when it was distilled.

Q. And your statement was first that it was formed in the redistillation—at least some of it is?

A. I said or in the condensation, in the same sense.

Q. Then you do not know that that is ever in the shale oil until an attempt is made to redistill it?

A. Yes, I do.

Q. Why did you say "or" then?

A. I was thinking of the commercial products as normally made, and the fact that probably part of it is formed under one circumstance, and in another portion of it the process continued the remaining.

Q. And part of it, you say, at least, is formed in the redistillation?

A. Yes.

Q. And is not in the shale oil at all, as it normally appears as shale oil?

A. A part of it is not.

Q. So that the asphalt figures you have given on shale oil do not mean that that asphalt was in the shale oil at all, but you made it in the process of redistilling it and testing it, in part?

A. No more than you would ordinarily with petroleums, in that respect.

Q. You know that the asphalt that is in petroleum is in there all of the time, do you not?

A. A part of it, yes.

Q. All of it?

A. No, not all of it, necessarily.

Q. Do you mean that when you steam a distillable petroleum, well petroleum, to an asphalt bottom, that you make any more asphalt?

A. I did not say anything about steam distilling, but I do know this—that if we will take some of our products from petroleum and expose them to the air——

Q. That is not what I am talking about.

A. (continuing). And redistill them, we get additional quantities of asphalt, which is a proof on your point.

Q. No, it is not by a city block. In the regular normal steam distillation of American crude to the asphalt bottom, do you or do you not make more asphalt?

A. I have never carried that out——

Q. Then you do not know?

A. (continuing). As a still operator.

Q. Don't you know?

Mr. Rauber: What is the question, again?

Q. Don't you know whether in the running of an American well oil to an asphalt bottom with steam, in the ordinary way, you make more asphalt?

A. It is my opinion that you do.

The Master: Can asphalt be synthesized in a petroleum product that contains no asphalt at all?

The Witness: Yes.

Q. In a steam distillation?

[fol. 1474] A. It can be synthesized, your Honor; we have done it—by adding a catalytic agent to it. That is something that simply speeds up the action. We recover our catalytic agent afterwards and that is one method that is being worked on now, with the idea of making it commercially. Apparently we can do it if we have a petroleum having the right kind of hydrocarbons such as some of the petroleums from the northern part of South America, Venezuela and so on.

Q. Will you just tell me the evidence of fact—I just want facts and not theories—upon which you dare tell this court that you believe that in steam distilling well petroleum to an asphalt bottom you make more asphalt than was there in the first place.

A. Because if you take a petroleum product, I mean a heavy petroleum product, and distill it again, you will get a small amount of asphaltic material left.

Q. In a steam distillation without air present?

A. It is very difficult to keep air out entirely, because air is present in steam normally—in steam from an ordinary power plant.

Q. In such quality as to increase asphalt?

A. A little.

Q. Do you mean to tell this Court we cannot take oils like lubricants, which are the next thing to your asphalts and run them over and over with steam and never touch the asphalt content and do not do it every day?

The Master: If you do not know, that answers the question.

Q. What work have you done personally, or what work has been done that is within your knowledge, giving the heat of reaction in cracking?

A. The work of E. E. Lyder, on which he obtained his Th. D., a thesis on shale oil——

The Master: That is not what he asked for.

The Witness: Oh, yes, your Honor.

Mr. Rauber: Yes, he did.

The Witness: He certainly did, your Honor. My personal knowledge.

The Master: Just keep quite for a minute. He is asking for your personal knowledge.

The Witness: This is my personal knowledge.

The Master: Did you assist Lyder?

The Witness: No, Lyder assisted me, and got his Ph. D. in return for it, and we determined these heats. I cannot give you at the present time just the number of them, what that heat was; but it is published in the Journal of Industrial Engineering Chemistry.

Q. That is shale oil?

A. Yes, sir, but that is the same as petroleum.

Q. And that gave off heat when it cracked, did it not?

A. It did not. It absorbed heat.

Q. How much?

A. I say I do not recall the numbers on that.

Q. Have you any other information of your own personal knowledge on the subject of the heat of cracking than that gotten on shale oil, which you have forgotten?

A. It was ten years ago or near that—nine years ago; and that is the reason why I do not have it in mind. I do not have a personal first hand knowledge of the details of other cracking experiments.

[fol. 1475] Q. If the heat of cracking were the thing that you have stated 900 b. t. u.'s per pound, you would get a drop of 500 degrees in the unfired drum of the Cross process, would you not?

A. No, you would get a drop of about 215, or a little over 200.

Q. How do you calculate it?

A. Because more than half of the cracking has been done in the tube, and less than half of the cracking has been

done in the drum. 900 represents, roughly—if you could say a yield of 25 per cent in the Cross process—it would be easier if we take it at thirty per cent, which is a little over statement. The thirty per cent of the oil has been cracked, and then you would have to divide your 900 by three to get the total amount of oil which is going to be changed in temperature. That would give you a little over 300, because it is a thirty per cent, or three and one-third. A half of that heat would be absorbed in the tubes, and half in the drum, if it is half cracked, and that would leave you, say 160, which would correspond on account of the low specific heat of about 150 or 160, and a low specific heat of oil to a drop of—well, I do not know what the specific heat is at those temperatures. But it is only around a couple of hundred degrees, 200 degrees Fahrenheit, or a little over a hundred degrees of Centigrade.

The Master: It is not a matter of actual measurement, but it is a matter of deduction?

The Witness: Yes.

Q. And of course in there you have vaporization and radiation coming in, too, at the end of this drop, this theoretical drop?

A. Yes.

Q. And what is the actual drop?

A. I have heard the statement made that it is 100 degrees. My own observation is that I think that is an underestimate of it, judging from the examination I made at a plant in Pennsylvania.

Q. The drop is about a third or a quarter of what your theory would tell you what it ought to be?

A. I do not know of any measurements that have been made there. In fact, I do not believe it has ever been measured.

Q. You were asked in your redirect about the Holmes-Manley process making only gasoline. I do not want any [fol. 1476] ambiguity about this at all. You said in your direct that these processes had to make both gasoline and kerosene. I call that to your attention yesterday and you said that is what you meant. Did you mean today that we could or we could not run a process like the Holmes-Manley and turn out of that apparatus only gasoline and tar?

A. I meant yesterday, and I started to explain my answer to it and you stopped me—that you could turn out only gasoline and tar, but that would mean that you would be recracking your kerosene. You stopped me when I was trying to make that statement.

Q. When you were asked a question about the reflux this morning, I understood you to say that the reflux from the dephlegmator at the top of the tower would be heavier than the material on the top of plate; was I right in that understanding?

A. Normally so, yes. I mean by heavier, in the case of gasoline and there; in the case of some other liquids——

Q. It would be higher boiling material than the material on the top plate?

A. Normally so. Theoretically you might have exactly the same, but it would depend on your proportion which you were returning.

Q. You have explained to the Court here at great length the operation of one of these towers and how the vapors are always lighter than the liquid in the pan, from which they came, and the vapors were always heavier than the liquid on the plate up above. Do you seriously mean to tell the court that you can run back into those top plates a liquid condensed out of the vapors above it, that came out of it, that is heavier than that liquid and will contaminate it?

A. When you run your dephlegmator there, you condense partially there; you condense out your heavier portions, including entrainment, to a considerable extent, and those heavier portions go down as liquid through your dephlegmator and your lighter portions go on to the lighter condenser for the lighter portions. And those heavier portions are normally heavier than your top plate.

Q. You mentioned the entrainment. That is identical with the liquid on the top plate, of course, isn't it?

A. Yes.

Q. So that bringing back the entrainment is not going to contaminate the top plate?

[fol. 1477] A. No, it would not so far as that phase of it, but your point is——

Q. Stop a minute. You have answered my question.

A. In your dephlegmation, you are going, perhaps to even pick out some of that liquid and allow it to volatilize, I mean the lighter ends of it, and the heavier portion goes back. There would be no point, if that reflux was not different from that top plate. There would be essentially no point in returning it.

Q. I would like to have you answer my questions, because every time you go into one of these rambling discussions you bring in something else that causes me to ask another question. Do you mean to say that can take a drop of liquid by entrainment from the top plate up into the dephlegmator which is colder, and vaporate some of it where lighter stuff is condensing?

A. Not where lighter stuff is condensing, but where your heavier stuff is condensing. It is a partial condenser, it is not a complete condenser. It is not a Humphrey's run-back.

Q. You cannot evaporate entrained particles, that have entrained from the lower plate up in that dephlegmator which is colder, can you?

A. When they are in contact with the hot other vapors they may give off part of their light liquids, leaving their heavy liquids to go back.

Q. Is there any fundamental difference in the relations between liquid and vapor, between dephlegmator top and the top plate and between the second plate and the top plate—any other two adjacent plates?

A. There is this additional difference—that ordinarily the velocity is not as high through there. There is partial condensation, and more condensation there. That is on a single plate ordinarily your amount of condensation is relatively small, and while we can figure, for example, that in distilling a particular product, two plates will do it and give us a certain purity, actually we find we have to use many more plates than that—ten or twenty.

Q. The general relations between the top plate of the dephlegmator are just the same as between the second plate and the top plate, are they not?

A. The general relations are.

Q. Exactly.

A. But not the complete relations.

Q. And when you partially condense, as you do, the vapors from the second plate on the top plate, you get a lighter liquid there than the liquid that was in the second plate, do you not?

A. Yes.

Q. And why doesn't the same thing hold true between the top plate and the dephlegmator?

A. How is that?

Q. Why doesn't precisely the same thing hold true between the top plate and the dephlegmator or partial condenser?

A. Because your conditions are quite different there.

Q. You condense more and you get lighter, don't you?

A. But you are not condensing—it is not simply a condensing of more of it; it is condensing the heavy ends and more efficiently.

[fol. 1478] GEORGE F. BENHOFF, a witness called on behalf of the Petitioner, in Rebuttal, being first duly sworn, testified as follows:

Direct examination.

Questions by Mr. Rauber:

I have been in the Refining industry for 30 years in various positions up to manager.

Q. On page 2402, defendant's expert said:

"The term cylinder oils, as used in any of the references, always means a residual product, which has never been distilled."

Do you know of any cylinder oils obtained from distillates?

A. At one time we had an occasion, with the Pierce Oil Corporation, to make cylinder stock for the Mexican trade. We were unable to get cylinder stock shipped to Mexico, and we were forced to make it from Mexican crude, and the only way we could obtain it from the Mexican crude was by taking what we called, down there, Panaco crude, and first distilled that, making a distillate, and then taking this distillate and redistilling the distillate, and the residue from the distillate we disposed of as cylinder stock.

The Master: Where was that done?

The Witness: In Tampico, Mexico.

The Master: Did you do that at any other place?

The Witness: No, sir, I never had any occasion to do it at any other place.

Q. On page 2576, defendant's expert stated, in answer to a question: "You can call gas oil or paraffin distillate kerosene, then?" To which he answered: "Yes." Have you ever known of gas oil or paraffin distillate being known as kerosene?

A. Never in my time have I ever heard it termed kerosene.

Q. On page 2574, defendant's expert was asked:

"Did you ever know of any illuminating oil that contained all of the gas oil fraction, particularly without the light ends?"

To which he answered:

"Yes, our mineral seal meets those specifications almost exactly."

Is mineral seal oil a gas oil?

A. A mineral seal oil is a cut between the kerosene and the gas oil. It don't contain all the gas oil.

Q. That is, in making mineral seal, you still have considerable quantities of gas oil?

A. Considerable quantities of gas oil.

Q. Has this mineral seal oil ever been known as kerosene?

A. Not to my knowledge. It is in the kerosene series. It is in the illuminating series.

Q. And not in the gas oil?

A. Not in the gas oil.

[fol. 1479] The Master: The witness says that he never knew of mineral seal oil being used as illuminating oil.

The Witness: It is in the illuminating series. It is a member of the illuminating series. But I never knew it to be known as kerosene.

The Master: Oh—then you agree with the defendant's expert, that mineral seal may be classed among the illuminating oils?

The Witness: It is in the illuminating series.

Q. But not a gas oil.

A. Not a gas oil.

Q. What grades of kerosene did you make prior to 1912?

A. We made what was termed water white, prime white, and standard white. Standard white, in some plants, was termed, common oil.

Q. Do you mean by your Company, or by the trade, generally?

A. By the trade, generaly.

Q. This common oil—where was that sold, mainly?

A. That was for the European trade. It was export oil.

Q. Did it contain any considerable quantity of the gas oil fraction?

A. It contained the light ends of the gas oil fraction. The gas oil was stripped and the light ends extracted from it, and it was then mixed with the heavy ends of the kerosene.

Q. You didn't ever put all of the gas oil into that export oil?

A. We never did.

Q. On page 2401, defendant's expert stated that the Saybolt and Engler viscosimeters are not used for the commercial measurement of kerosene. Is that statement correct?

A. Well the Saybolt is only used in cases where the specifications call for viscosity, but I wouldn't say it is commercialy used on all kerosene.

Q. But is it ever used in the commercial measurement of kerosene?

A. It is, at times.

Q. And the Engler viscosimeter, also?

A. Well, I wouldn't say anything about the Engler, but I know the Saybolt has been used.

Q. You don't know about the Engler?

A. No.

Q. On page 2573, defendant's expert stated that "A great portion of the gas oil cut, in the old times, was used for illuminating oil." Is that statement correct?

A. There was a portion of it that was stripped—the gas oil was termed top, and was stripped, and that portion that was stripped off was used in the manufacture of what we termed common oil, or standard white.

The Master: Are you speaking of your own practice, in the positions in which you testified you were engaged, or are you speaking generally of the trade?

The Witness: That is, to my knowledge, it was the practice of the trade to do that—to strip the gas oil and use the tops in the manufacture of their standard white, or common oil.

Q. What proportion would you take out of the gas oil, in making those strips?

A. We would strip anywhere from 20 to 30 per cent.

Q. And leave 70 per cent. of gas oil?

A. 70 per cent. of gas oil.

The Master: And you are confining your testimony, now, of course, necessarily, to that period of time beginning with about 1894?

The Witness: '94 or '95.

The Master: And you are not presuming to speak as to what was done by the trade in respect to kerosenes manufactured before 1894 or '95?

The Witness: No, sir.

Q. On page 2573, defendant's expert was asked: "You would put all of your gas oil in your kerosene?" To which he answered: "Yes, they ordinarily did." Was that answer correct?

A. I have never known it to be done at any plant that I was ever employed in, and I never heard of it being done.

[fol. 1480] Cross-examination.

Questions by Mr. Wiles:

Q. You speak of the use of the Saybolt viscometer, or viscosimeter, on kerosene—when you use that what does it read?

A. Well, now, it reads in seconds and tenth of seconds, and in taking your reading you take your—say, for instance, if it was 32 and three tenths, at 70, and it is corrected back to 60, which would give you probably—it has been a long while since I saw it run, but I would say it would give you probably about 360 or 380, at 60—or something like that.

The Master: When you say 60, you mean temperature?

The Witness: 60 temperature, yes, sir. It is all corrected back to sixty.

Q. Now, how many seconds does kerosene ordinarily run, on the Saybolt viscometer, that you test it with?

The Master: At what temperature?

Mr. Wiles: Corrected.

The Master: At 60 degrees?

Mr. Wiles: Yes, sir, at 60.

A. Well, roughly, around 35—35 to 36—somewheres in there.

Q. Now, will you please describe the Saybolt instrument that you say you used, or is commonly used on kerosene?

A. Well, you are supposed to have your room temperature——

Q. No, just describe the instrument.

A. Well, you have a cylinder—a glass cylinder, as a container for your oil. Then you have a——

The Master: What is the diameter of the tube, or cylinder?

The Witness: I would say approximately an inch and three-quarters. Then you have a capillary tube.

Q. A glass capillary tube?

A. Yes.

The Master: At the lower end?

The Witness: No. That extends down into the oil, with a bulb on top of this tube, and your cylinder is filled to your proper height, and then your bulb—you put your finger on top of your bulb, and your bulb is pressed to force the oil out of this capillary tube, and as you get all the air out of the tube you release your finger and start your stop watch at the same time, and as the oil reaches the top of the tube you take your reading.

Q. This glass capillary tube instrument that you use, you used on kerosene, and it is not the Saybolt universal viscometer, or viscosimeter that is employed on lubricating oils?

A. No, sir, it is not.

Q. It is the so-called theremo viscometer, that is used on kerosene especially, is it not?

A. Yes, sir.

The Master: It is not the Saybolt viscosimeter that we have been talking about?

Mr. Wiles: Exactly—Mr. Wilson said that there was another one, and that that was the one that was used.

Q. You have testified about gas oil as made in the days when you were in the industry practically, and that included the practice in the Standard Oil Company in Baltimore, did it?

A. Yes, sir.

Q. The gas oil you were speaking of came principally from coking stills?

A. Especially from coking stills.

Q. Did you do any running for steam refined there?

A. We ran for steam refined.

Q. So you had some gas oil from steam refined and some from the coking stills?

A. Yes, sir.

Q. But most of it from coking stills?

A. Most of it. That at that time originally was termed the old tar still. That was prior what they term the coking still today.

Q. Well, it was a coking still?

A. It was a coking still, yes.

Q. You ran down to a hot bottom in the usual way; it differed in its overhead principally from the present tower or Van Dyke type of still?

A. Yes.

[fol. 1481] Q. In running those coking stills you got off first your gasoline cuts, roughly,—it may have been subdivided and parts of it re-run, but broadly you got off a gasoline cut first; that is right, isn't it?

A. Yes.

Q. Then following that, perhaps with an intermediate cut, same your kerosene; that is correct, is it?

A. Well, no. No, that is, we took off our gas—that is, they were not both taken off at the same time.

Q. No, I mean in succession.

A. Yes, in succession.

Q. You first got off a gasoline, broadly speaking. You may have had a steam still stock cut in between, which we can forget, because it went in either gasoline or kerosene. But you first get gasoline, and as the temperature went up you got kerosene?

A. That is it.

Q. When you got through your gasoline or water white cut, as you call it—is that what you call it, by the way?

A. Water white, yes.

Q. And then you went to gas oil, that is right, isn't it?

A. Not when we had the tar stills, we did not.

Q. What was your next cut?

A. We had just taken off our gasoline and kerosene cuts; then it went direct from there to what we termed our tar stills.

The Master: You called it what?

The Witness: Tar stills.

Q. Oh, you coked in separate stills?

A. In separate stills.

Q. You took off your gasoline and kerosene continuous?

A. Oh, sometimes we did it continuous, other times we did it batched.

Q. You skimmed first?

A. We skimmed first.

Q. I see. Then you took your reduced crude to your coking stills?

A. To the coking stills.

Q. And your first cut there was gas oil?

A. Our first cut there was what we termed crude equivalent. It was in the gas oil series but contained a large percentage of naphtha.

Q. That is when you were cracking?

A. No, the original tar stills had what we termed large [fol. 1482] aerial condensers that worked practically on the same principle as a tower does today, and the vapors were flashed up into this aerial condenser, and the vapors there were separated and fractionated and the heavy end of the vapors trapped back into what we term a heavy drum, and the intermediate vapors and the condensation from it trapped back into our intermediate drum, and the product that went comp-etely over the top, or residue vapors, were termed crude equivalent. It was the gas oil series, and it had considerable naphtha in it. It had to be first distilled to get the naphtha out of it.

Q. You re-ran that?

A. We re-ran that.

Q. But your first lightest cut in the crude stills was essentially gas oil, with some naphtha in it?

A. Yes.

Q. That had not been stripped out in your earlier stills?

A. Yes.

Q. After your gas oil you began running paraffin distillates?

A. Paraffin distillates.

Q. And you determine the point between gas oil and paraffin distillate by the point at where you want your pressing stock to be right?

A. Well, the way we determined that, we determined that with ice. If you take a drop of it on ice, if it chilled immediately, we would cut it.

Q. In other words, you ran gas oil until you began to have visible quantities of paraffin.

A. Visible quantities of paraffin.

Q. Then you cut the paraffin distillate?

A. Yes, sir.

Q. You took this crude equivalent cut and you re-run that, skimmed it, and part of that went to gas oil?

A. The residue was gas oil.

Q. Then you took your paraffin distillates, cut and chilled and pressed?

A. Yes, sir.

Q. And then you steam reduced the pressed drips for lubricants?

A. Not always. At times we would throw the pressed distillate back into gas oil.

Q. At times it all went into gas oil, all the pressed drips?

A. All the time.

[fol. 1483] Q. That would be when you were making no lubricants?

A. Making no lubricants, no.

Q. But when you were running lubricants you steam reduced the pressed drips?

A. Yes.

Q. And about 80 per cent of the pressed drips which went overhead, perhaps eighty-five per cent would go back to gas oil?

A. Oh, no, not over——

Q. Seventy-five per cent?

A. Not over forty-five per cent.

Q. As much as half of the paraffin distillate was suitable for lubricants in your practice?

A. Not half of the lubricant distillates but half of the pressed lubricant distillates.

Q. Then you did not follow the practice that many of the refineries did, of including a good deal of lighter material to make your presses run easily? You know that practice, do you not, that I am alluding to—leave more gas oil and paraffin distillate to run your presses easily; you are familiar with that practice, are you?

A. Well, the only practice that I ever followed, or ever saw followed, in making paraffin distillates, is viscosity. They tried to hold to a certain viscosity. In all my practice, with a viscosity of about sixty-five, if you got good crystals in your paraffin I always found it was the best paraffin to attempt to press.

Q. And you leave whatever gas oil you need in, then, to give you sixty-five viscosity?

A. Yes.

Q. In addition to the two sources of gas oil that you named—that is, that which came from this crude equivalent and that which came from what we call paraffin light ends—you understand that term?

A. Yes.

Q. You had those two sources of gas oil, did you not?

A. Yes.

Q. You had another source of gas oil in re-running your kerosene, did you not?

A. Well, we never termed that the gas oil, because it was dark, and we always termed it light fuel oil, and we never did put it into gas oil.

Q. Never did put it into gas oil?

A. Never did put it into gas oil.

[fol. 1484] Q. That would be distillate fuel oil?

A. Distillate fuel oil.

Q. Comparing the gas oil that you got from re-running this crude equivalent with the paraffin light ends, how did they compare in quantity from the same crude?

A. About 75 per cent of your straight gas oil, and roughly speaking, about 25 per cent from the other source—probably not that much.

Q. When you ran for steam refined, did you follow the

same general practice? You got off a gasoline cut, roughly, and a kerosene cut, and then some gas oil?

A. And gas oil.

Q. And then paraffin distillate?

A. Then we got what we did not term a paraffin distillate, but termed a neutral distillate, because the wax was not a crystallized wax. We had to take that back and crack it to form a crystalized wax.

Q. It would not press?

A. It would not press, no.

Q. You ran it in a straight fire still?

A. Straight fire still.

Q. Then pressed it?

A. Then pressed it.

Q. Then that went right into paraffin distillate?

A. Went right into paraffin distillate.

Q. So from that you also had the two sources of gas oil?

A. Yes, sir.

Q. The gas oil that came directly from crude as it were, and that which came back, and the overhead from the paraffin stills?

A. Yes, sir.

Q. In this plant were you running for the maximum possible lubricants?

A. Well, that always depended on the market and just depended upon—of course, at times, there are lots of light lubricants, a- times you can dispose of all the light lubricants you can make; but at times you cannot dispose of them, and at that time you take the heavier fractions of your lubricating oils and throw your light ends back into your gas oil again.

Q. And of course in that case, if you had this surplus, a good deal more than 45 per cent would go back into gas oil—45 per cent paraffin distillate?

A. I do not think the average over a year would be much over 45 per cent.

Q. Yes, but at times?

A. At times, yes. At times it would be.

Q. At times your gas oil from paraffin distillate would be a very large part of the total gas oil. At times it would be, yes.

The Master: In describing these processes, which of the plants in which you were employed did you have in mind?

The Witness: Well, this practice was principally followed by the Standard Oil Company in Baltimore, the Indian Refining Company, and the Magnolia; but at the Magnolia we did not have tar stills, we had down there what we call tower stills where the operation was completed in one still.

[fol. 1484½] Earle Morse, a witness on behalf of the Petitioners, after being duly sworn by the Master, testified as follows:

A witness on behalf of Petitioner, testifying as follows. At the request of Dr. McKee, I took samples of gasoline from commercial filling stations around New York in November, 1925. I produced the samples which are marked with the stations from which they were obtained. When obtained, the samples were all off color, none being a water white. The present color is darker than when obtained except in one possible case. In one or two cases, the yellow color has dropped down to the bottom. There was no gum in the gasoline when obtained. The samples produced are as follows.

(The said exhibits so offered were admitted as physical exhibits, and marked as follows:

Bottle with label "Texco. 196 Clinton Ave., Irvington, N. J. 11/12/25. E. H. Morse" marked Exhibit No. 615.

Bottle labelled "Texaco, 1148 Springfield Ave., Irvington, N. J. 11/12/25, E. H. Morse," marked Exhibit No. 616;

Bottle labelled "Tydol, 1139 Springfield Ave., Irvington, N. J. 11/12/25, E. H. Morse," marked Exhibit No. 617;

Bottle labelled, "Purol, Elmwood & Springfield Ave., Irvington, N. J. 11/12/25, E. H. Morse," marked Exhibit No. 618;

Bottle labelled, "Std. of N. J., 1239 Springfield Ave., Irvington, N. J. 11/12/25. E. H. Morse," marked Exhibit No. 619;

Bottle labelled "Std. of N. J., 11/12/25,—E. H. Morse," C. C. Shaw Hdw. Chestnut St., Nutley, N. J." marked Exhibit No 620;

Bottle labelled "Std. of N. J. Chestnut St., Nutley, N. J. 11/18/25, E H. Morse," marked Exhibit No. 621; and

Bottled labelled "Sinclair Gas & Oil Co. 824-36 Springfield Ave., Irvington, N. J. 11/12/25. E. H. Morse," marked Exhibit No. 622,

and the same are herewith returned.)

Mr. Wiles: No cross-examination.

[fol. 1485] WARREN K. LEWIS, a witness on behalf of the defendants, being recalled, in sur-rebuttal, testified as follows:

Direct examination.

Questions by Mr. Wiles:

Q. Dr. Lewis, have you examined the tables, 27 and 29, which Dr. McKee referred to, in Bacon and Hamor?

A. Yes, sir.

Q. Those tables show a large number of so-called asphalt determinations by the Holde method, which is described in the book.

The determinations, as outlined in the book, consisted in taking the residuum, as run in those experiments—which, however, included the gas oil and lubricating cuts, as well as the ultimate residuum—and determining the fraction of this residuum, which was insoluble in gasoline of low boiling point, and consisting, only, of saturated hydrocarbons.

Now, in all ordinary American petroleums a very large fraction of the residuum left, on complete distillation by ordinary means, either with vacuum, or steam—a large fraction of this residuum is, I say, soluble in petroleum ether.

Inasmuch as the figures given in table 29 of Bacon and Hamor, include only the insoluble in gasoline, it, of course, follows that they represent only a fraction of the residuum.

[fol. 1486] Sometimes that fraction is high, and sometimes it is low. We have petroleums in which the residuum is almost completely—is completely soluble in gasoline. We have other petroleums in which a good percentage of that residuum is not soluble. Those figures on asphalt, as they are quoted and interpreted by the analytical method described, do not throw any light on the amount of residuum, unless you know the character of the residuum, and of the crude from which the residuum came.

Q. When you say "residuum," in the last part of your testimony, you mean undistillable residuum?

A. Yes, sir. It is important to distinguish there, because those tables report, as residuum, in the column describing residuum which means the residuum which they left in the still, and included all the gas oil and all the lubricating cuts, as well as what I shall describe as the true residuum. The true residuum is the residuum which would have been left had they completed the distillation by the use of either vacuum or steam, to as high a temperature as they could go without thermal decomposition.

In the asphaltic base oils that true residuum is ordinarily referred to as asphalt, and a very large proportion of that true residuum, so called asphalt, is usually soluble in saturated gasoline.

Q. So, in brief, those tables give no light on the point that Dr. McKee used them on.

A. These tables throw no light on the true residuum that would have been found had these oils been analyzed for true residuum, by reduction with either steam or vacuum, to the highest temperature allowable without thermal decomposition.

Q. By subtracting that so-called asphalt determination from the unsaturation of the whole crude, do you get any light on the unsaturation of the distillable parts of the crude?

A. No, because that difference includes the unsaturateds in the true residuum and the gasoline soluble portion of that true residuum always carries large percentages of unsaturateds soluble in sulphuric acid.

Q. Petitioner's expert has stated several times that petroleum and shale oil were identical, and also that no chemist could tell definitely by analysis or any other means

whether a given oil was a shale oil or a crude well petro-
[fol. 1487] leum; and he even extended his statement on page 9489 to include Mid-Continent petroleums. What have you to say on the possibility of a chemist distinguishing between shale oils and well oils, and particularly Mid-Continent petroleums.

A. American Petroleums in general and Mid-Continent petroleums in particular are characterized by the fact that they contain, especially in the distillable portions and most particularly in the lower boiling fractions of the distillable portions—that is to say, the gasoline and kerosene cuts—very small percentages of olefines; on the other hand, shale oils are characterized by high percentages of olefines. Therefore, it follows that by examining the sample of oil submitted to determine the percentage of olefines—that is to say, the unsaturation as determined either by sulphuric acid or bromine acid addition or iodine number, if that is desired, although it is less satisfactory—one can distinguish between well petroleum and shale oil, because the well petroleum in the gasoline and kerosene cuts will show low percentages of unsaturateds by these tests, whereas, the shale oil will show higher percentages.

This is substantiated by the data shown on page 101 of Dr. McKee's book, which has, I understand, already been put in evidence. On that page are shown analyses of the distillable portions of both shale oil and typical crudes, and the shale oil distillates run from 28 to 40 per cent, as I recollect it, whereas, the crudes run from three to four per cent, in percentage of unsaturateds in the distillable portions.

The Master: In that comparison, do you have in mind, the well petroleum of the United States nearest to shale oil in its olefine contents?

The Witness: Yes, I know of no evidence indicating a high percentage of olefines in the distillable portions of well petroleum—of any ordinary well petroleum anywhere. There may be freak well petroleums that have high percentages of olefines. I have never seen such an analysis, and I have examined them, not only the analyses of Bacon & Hamor, but analyses of tests on well petroleum, and the distillates from them.

That is substantiated by the fact that it is well recognized that the percentage of unsaturateds in straight run gasoline taken off without cracking is low, and every refiner knows it is low.

Q. Dr. Lewis, have you examined the drawings and construction of the Pielsticker patent?

[fol. 1488] A. Yes, sir; I have it before me.

Q. Dr. McKee, Petitioner's expert has stated that in his opinion the furnace of the first coil which is shown in Figure 2, and the furnace under the main drum—not this vapor phase coil, which is a third coil, only sometimes used, but the furnace under "L"—would be of the same size and would normally develop the same amount of heat, and we would normally fire them to the same extent. I wish you would make that assumption and state what would be the relative temperatures at the end of the coil and in the drum or re-action chamber L, with that kind of firing.

A. In answering your question, from reading Pielsticker's description, it is my understanding for me to assume a cold feed stock, fed through his first coil, discharging from that first coil into his drum, with substantial equal conditions of firing in the two fire boxes indicated in his drawings.

Q. Yes.

A. Under those conditions it would of necessity follow that approximately the same amount of heat would be picked up in the two units of apparatus. If you fire two furnaces to the same extent and in the same way, two furnaces such as these which the drawings indicate are very similar in construction and general layout, the radiation and conduction losses, which are small anyway, will be substantially the same and the only differences in heat picked up, the essential differences in heat picked up, in those two furnaces will depend only on the amount of heat lost in the stack gases in the two cases.

Both these furnaces, while they are represented by diagrammatic drawings, are evidently laid out by a man skilled in the art. They are so sketched as to indicate clearly that the combustion gases after leaving the fuel bed are brought into intimate contact with the body being heated. That means that in both cases the furnace gases

would be cooled to a reasonable degree. Furnaces of this type, as built at that time, did not have high furnace efficiency. It is true that the coil furnace would cool the stack gases to a somewhat greater degree, because there is, roughly, three times the heating surface in the coil that there is in the drum; on the other hand, it takes a tremendous difference in heating surface and method of operation to make much of a difference in furnace efficiency, and I should estimate that there would not be over ten, or at the outside fifteen per cent difference in furnace efficiency between these two furnaces. That would mean that [fol. 1489] both furnaces would take up nearly the same amount of heat, the coil, however, taking up a little more than the drum. When you put heat into a body, something has to happen. You are putting, let us say, at the wide outside, forty parts of heat in the drum and sixty parts into the coil. I do not think the discrepancy would be that much, but certainly forty parts into the drum and sixty parts into the coil. In the coil the heat goes primarily to raise the temperature. What does it go to do in the drum? In the drum it is true a certain amount of vaporization takes place, but the heat of vaporization of these materials at these high temperatures is small. In the drum cracking takes place, and a certain amount of heat is consumed. But all the evidence is that the heat of cracking is small. The temperature of the drum must rise a substantial degree, and while the temperature rise in the drum will not be quite so high as it is in the coil, there will be substantial temperature rise. Granting a feed stock at room temperature—let us say seventy degrees,—and a drum temperature of 770, that is a 700 degree temperature rise at the very least 200 degrees of that, and probably nearer 300, will take place in the drum. That would mean that at the coil outlet the temperature would be somewhere between 470 and 570, and I think that those figures are low and not high.

Q. And if you raise to such a temperature as that in the coil, can you get carbon deposit?

A. Carbon deposit at 570?

Q. Yes.

A. Even 470?

Q. Yes.

A. You certainly can under special conditions, and in time you will get it anyway.

Q. What have you to say on the last point I examined Dr. McKee about in re-cross examination, the relation between the reflux liquid, and the liquid on top of plate in a bubble tower? Is the reflux liquid heavier than the liquid on the top plate so that it would contaminate it?

A. The reflux liquid from the dephlegmator above a bubble tower on to the top plate cannot possibly be heavier than the liquid on the top plate. In a bubble tower we have a progressive lightening of the liquid all the way from the bottom of the tower up to the top, and that lightening does not stop between the top plate and the dephlegmator, but [fol: 1490] the dephlegma-or in turn condenses heavier fractions and returns them back.

But the whole function of the bubble tower is to produce this lightening, to have conditions such that there must be a light liquid in upper portions and a heavier liquid in the lower portions. You simply do not have a condition reversing that. Now, for example, let us take a liquid on the top plate. Suppose it does splash up into the dephlegmator. It is still the liquid on the top plate. But in the dephlegmator it comes into a cooler zone—that is what the dephlegmator is; it is a cooler on top of the column—it comes into a zone therefore, where condensation is taking place. But the stuff that in condensing, while it consists of a considerable portion of heavy fractions, brings down also a considerable portion of light fractions, and the ratio of light fractions to heavy fractions is of necessity greater than the ratio in the liquid on the top plate. The consequence is that due to this condensation the liquid droplets splashing up into the dephlegmator, gets lighter, and when they run back, they run back as material lighter than the liquid on the top plate.

Let us consider the vapor that rises from the top plate, aside from any entrained drop going up with it. That vapor when it starts to cool, starts to deposit at the start a liquid of the same condensate as the liquid on the top plate and of the same density, but that is only a differential in amount, and the very next fraction that goes out, is somewhat lighter, the further you go the lighter the condensate. The reflux into the top plate of a column from a dephlegmator, both in theory and in fact is lighter, always lighter than the liquid on the top plate.

Q. And that is why you do not want to put it somewhere down the column where it won't be like the liquid?

A. That is why it ought not to be introduced at a lower point in the column.

Cross-examination.

Questions by Mr. Rauber:

Q. Dr. Lewis, when you calculated the heating surface of the drum and the coil in this Pielsticker patent, did you calculate the entire surface around the retort?

A. I tried to estimate that. As I understand it, such drawings as this are diagrammatic. You will note that on the drawing on page 1669 he has his drum in juxtaposition [fol. 1491] to his second coil. On the other hand in the description he says that the second coil is similar to the first, and consequently so far as ratios of heating surfaces are concerned, I can use this sketch on page 1669.

Now, then, for example, suppose that drum were one foot in diameter. If it were, it would be a little over three feet in circumference. Now, then, if you turn back to page 6166, you will find the billets in which his coils are embedded. The outer surfaces of those billets form the heating surface from which heat is picked up from the gases. You will find he shows four such billets in this furnace, and those billets are, roughly, of the diameter of his drum, so that the sides of those billets represent, roughly, eight feet—eight as compared with three. The end corrections are minor, but roughly 3 to 1 is the ratio of the heating surface.

Q. And you counted the entire circumference of the retort as the heating surface?

A. Yes. I just said if you assume that the retort is a unit in diameter its entire circumference is a little over three feet—3.1459, something like that.

Q. That would be the usual way to count the heating surface on a retort or still, would it?

A. No, ordinarily the flame does not play up around the whole still. On the other hand in this particular still apparently it does. Looking at the diagram on page 1667, he does not show in any detail the setting of his retort in the furnace. But looking at that sketch on page 1667 it appears there that the flame is intended to play up over the top of the retort just exactly as it does over the top of his billets.

I have rather assumed that his retort was run pretty full, and that most of his heating surface was covered with liquid. Of course there is a top portion there which is not covered with liquid, but apparently his flame actually plays around it, so it would pick up a certain amount of heat from the flame.

Q. There is no showing whether it plays around it or not, except from those dotted lines.

A. No, it certainly is not definite, but it looks that way to me.

Q. Ordinarily you would count the entire circumference of the still, then, as the heating surface?

A. No, ordinarily you count the area over which the flame plays, depending on the setting of that particular still, the way that still is constructed. Frequently it is as little as [fol. 1492] one-third of the still circumference, sometimes it is as much as sixty per cent.

Q. In the upper part of that retort where the vapors come in contact with the walls you do not have as good a transference of heat from metal to vapors as you do from metal to liquid?

A. That is, usually you do not.

Q. And you do not get as good a transfer from metal to liquid as you do when the liquid is traveling very fast past the metal, do you?

A. From metal to liquid, yes.

Q. Especially when there are bends to churn the liquid up?

A. Whenever you have anything that will give high velocity or agitation or turbulence to the liquid past the metal, you get better heat transference from liquid to metal.

Q. So far as the transfer of heat from metal to liquid is concerned, the heat conditions are more favorable in the coil in this instance than they are in the retort, aren't they?

A. Yes.

Q. And how high in unsaturated constituents would the fractions between 150 and 300 degrees Centigrade be in Mid-Continent oil?

A. In mid-continent oil? Oh, two to five per cent. Eight, maybe.

Q. On page 145, there is one given as fourteen per cent, if I read it correctly. Do you know whether that is right or not?

A. Page what?
Q. Of Bacon & Hamor?
A. Of Bacon & Hamor.
Q. Yes.
A. No.
Q. You know it is wrong?
A. No, I did not determine it.
Q. You do not know whether it is right or not?
A. No, I cannot say, surely.
Q. It might be that high?
A. It might be that high, especially if there is a little carelessness used in distilling off the fractions and the temperatures are allowed to get up so that cracking started.
Q. Do you class the New Mexico oils as Mid-Continent?
A. Why, I should not think so.
Q. What are Mid-Continent oils?
A. Why, I have always interpreted Mid-Continent oils as [fol. 1493] oils from the Mid-Continent field—north Texas, Oklahoma, Kansas.
Q. Do you know how low shale oils could possibly run in unsaturateds?
A. No, I cannot qualify as a shale oil expert. Dr. McKee's book indicates that they run high, twenty-five per cent and up for the distillates particularly.
Q. That is the usual run?
A. So far as I understand it. But I cannot qualify as a shale oil expert. I have never done any of that work in my life.
Q. You do not know whether there are shale oils that will run any lower than that in unsaturateds?
A. I just said I do not know. I am not a shale expert. I just don't know.
Q. Going back to that tower, the liquid that goes up from the top plate into the dephlegmator, some ends that are—does it leave any ends than are higher than the average liquid, on the top plate?
A. Certainly.
Q. If you condensed out just those ends, mainly, just a light cooling—could you do that?
A. Your question referred to the liquid that went up from the top plate to the dephlegmator. It is condensed already.
Q. The vapors that go from the top plate to the condenser, would they contain any heavy material—that is, heavier than the average gravity of the material on the top plate?

A. Certainly.

Q. Could you condense out those heavier materials?

A. Yes, you can, but they always carry more light than there is on the top plate—always.

Q. Regardless of how you condense them?

A. Yes. At the start they start off carrying down just the same, but the only quantity that goes down that way is what we call a differential quantity. A differential quantity is smaller than any assignable quantity, however small. The minute you start to condense a quantity larger than any assignable quantity, however small, the condensate is lighter than the liquid on the top plate, always.

Redirect examination.

Questions by Mr. Wiles:

Q. Dr. Lewis, Mr. Rauber asked you about the transfer from metal to liquid in these stills. That is not an appreciable part of the difficulty of heat transfer, is it, the difficulty is from gas to metal?

[fol. 1494] A. The transfer from metal to liquid, in these stills is inconsequential, practically, because you have to send that heat through two resistances. The first resistance is gas to metal on the outside of the metallic wall; the second resistance is metal to liquid on the inside of the metallic wall. The resistance from gas to metal on the outside of the metallic wall is many many times greater than the resistance from metal to liquid on the inside of the wall. The result is that whether the resistances on the inside of the wall be great or small is simply inconsequential. The thing that counts is the resistance gas to metal, on the outside. That will be a little bit better around the billets than it will around the drum. But, on the other hand, the difference between the billets and drum will not be very great, and I really think that my estimates, are conservative.

Recross-examination.

Questions by Mr. Rauber:

Q. I would like to ask you one more question in regard to that. When speaking of the transfer of heat from gas to metal, you had in mind conductance, did you not?

A. Conductance, yes.

Q. There are other ways of transferring it?
A. There are; particularly radiation.

Mr. Wiles: I want to offer as "Exhibit No. 654," the photostats of the pages of McKee on Shale Oil, to which I called attention. There are nine sheets.

The said photostats so offered, were marked "Exhibit No. 654," were admitted in evidence, and the same are herewith returned.

[fol. 1495] IN UNITED STATES DISTRICT COURT

[Title omitted]

ORDER APPROVING NARRATIVE STATEMENT OF PATENT EVIDENCE

The foregoing narrative statement of the patent evidence approved this 7th day of August, 1930.

John Lord O'Brian, Charles H. Weston, Solicitors for Appellee United States of America.

Approved this 18 day of August, 1930.

Evan A. Evans, George T. Page, United States Circuit Judges, Sitting in the Above-entitled Cause.